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THE PSYCHOLOGICAL REVIEW

THE INTERRUPTION OF TASKS

BY W. C. H. PRENTICE

The Johns Hopkins University

Psychologists still know relatively little about the influence of interruption on behavior. Such a statement made baldly in the face of so considerable a literature as has resulted from the early experiments of Zeigarnik (20) and Ovsiankina (11) may meet with some objection. And yet an examination of that literature makes it apparent that certain contradictions and inconsistencies have prevented the construction of any theory having the twin virtues of completeness and simplicity. The present paper will examine some of the seemingly contradictory facts and discuss experimental and theoretical devices for coördinating them.

Zeigarnik gave her subjects simple operations to perform and then interrupted the performance of certain tasks in the series before completion. In an informal recall test, subjects remembered the interrupted tasks better than the completed ones in a ratio of 1.9: 1. Control experiments enabled Zeigarnik to discard most of the more obvious explanations of her data, and she arrived at the conclusion that the need (or 'quasi-need') to complete the task as instructed produces, within the person, a tension system which is not ordinarily resolved until the original need is satisfied. When a task is interrupted, the tension persists, producing forces which direct the subject's thought toward the incomplete task.

Ovsiankina attacked the same tensions from another point of vantage. If tension systems within the person correspond to the presence of an intention, and if such systems persist after interruption, the subject's 'quasi-need' to work on the task ought also to persist. Ovsiankina investigated the tendency for subjects to try to resume or continue the tasks on which they had been interrupted. In a thorough investigation of a great many variables, this experimenter was able to show that the trend toward resumption of interrupted tasks is a clear and fairly predictable phenomenor.

The writings of Lewin and his students (*e.g.*, 8, pp. 98–102, 170) have consistently implied that the force which produces resumption and the force which produces enhanced recall arise from the same tension system, and are thus indirect measures of the same thing.

What confusions have developed within this accepted theory? One difficulty concerns the relation between resumption and recall, for some evidence points to the conclusion that they may be expressions of different psychological forces. Another obstacle to clear thinking in this field is the difficulty of distinguishing between the effects of interruption *per se* and the effects of failure; for when a task has been interrupted short of completion, the subject may experience the hindrance as an indication

of personal failure, and when he does, the effects which appear are not easily incorporated into the theoretical scheme originally proposed by Zeigarnik. The literature, moreover, contains apparent contradictions concerning the influence of levels of importance or 'reality' in the interrupted tasks. Difficulties arise in practice when one tries to make experimental use of Zeigarnik's corollary generalizations, particularly as they apply to the influence of later activity. Finally, under particular circumstances, enhanced recall simply does not appear!

1. *Recall and resumption.*—One anomaly that looms very large in the theory of interruption is the experiment reported by Rosenzweig (16, p. 480) in which children who tended to resume interrupted jig-saw puzzles tended to recall the completed ones better than the interrupted ones. The Lewinian theory held, of course, that a subject who has a strong tension system resulting from the interruption of a task may be expected to show two correlated effects: a tendency to resume the task and a tendency to think about it or recall it. But Rosenzweig's subjects show an inverse relation between resumption and recall and consequently cast some doubt on the usual formulation. Rosenzweig's own account of the reactions of his subjects is presented in terms of 'success' and 'failure,' a terminology which further complicates the basic theoretical picture.

2. *Success and failure.*—The distinction between interruptions which are viewed as failures and those which are either neutral or apparently successful is not a new one. Ovsiankina (11) found it necessary to point out that ambitious subjects view the experiments as an opportunity for trying their ability, with the consequence that such subjects will resume those tasks which promise success and avoid those which promise failure. If this is true, the *expectation* of success or failure is an important variable in the resumption situation, and the determinants of such expectancy will need thorough understanding before we can make successful predictions about the outcome of experiments involving interruption.

Recent researches have not solved the fundamental problem, but they have thrown some light on the nature of the reaction to failure. Rosenzweig, for example, reports the following facts.

(1) Preference for resumption of failed tasks increases with age in children (14).

(2) Adults given jig-saw puzzles as an informal task tended to recall more easily those which were interrupted, while others given the same tasks as 'intelligence tests' recalled completed tasks better. The latter group presumably experienced the interruption on serious tasks as failure and 'repressed' the unpleasant memory (15).

(3) Children whose teachers rate them high on the trait of pride recall successes better than failures, while children rated low on the same score recall more failures than successes (17).

(4) Those children who choose to *resume* failed tasks *recall* their success better than their failures, whereas those who repeat the previously successful tasks remember mostly failures (16).

These results may be fitted into a consistent picture. It would seem that if we hold the situation constant and vary the type of subject (younger vs. older, or those low in pride vs. those high in the same trait) we find two trends. First, the mature and the proud tend to 'repress' the memory of their failures, as a result, presumably, of a strong need for success or for social status. Second, these same subjects, for the same reason, show a tendency to resume those failed tasks in order to satisfy their pride and to remove the previous defeat. If, on the other hand, we hold the type of

subject constant and vary the nature of the situation, we find a trend that dovetails nicely with the personality factor. For tasks which are important to the individual (that is, touch his pride or affect his own view of his social status) are repressed, while those tasks which are unimportant seem to be affected only by the simple interruption-effects originally reported by Zeigarnik. In sum, any threats to the subject's view of himself as a successful individual will produce a tendency to forget those threats, but also to overcome them. The mature subject, or the subject who has sufficient pride will be more sensitive to such threats, and 'important' tasks will be more threatening when failed than will incidental ones.

These conclusions are consistent with those of Cartwright who asked his subjects to rate a series of tasks in order of preference (in place of the usual resumption technique) and who reports: "A fairly sizeable group of subjects raised the preference-rating after failure,[1] thus demonstrating the necessity for a theory of failure which can account for both increases and decreases in attractiveness. Interviews with the subjects seemed to indicate clearly that many subjects who raised their preference-rating viewed the experimenter's evaluation of their performance not as failure, but rather as an obstacle in their way to the goal. . . . Other subjects seemed to want to 'erase' the failure by subsequent success in the same activity (3, pp. 11, 12)." Of this latter group, Cartwright comments in a footnote (3, p. 12), ". . . it is possible that one could speak of a 'need for success.'" In terms of Rosenzweig's formulation, Cartwright's subjects who preferred tasks on which they had previously failed should be those with strong 'ego-defenses.' They are presumably the mature and the prideful subjects.

Cartwright's final paragraph reveals, however, a further complication. He writes: "In conclusion, there is some indication . . . that anticipation of failure more unequivocally reduces attractiveness than the actual experience of failure. This fact suggests that a person's view of his future is the primarily decisive factor in producing changes in attractiveness (3, p. 13)." If true, this fact necessitates our dealing with different levels of aspiration among our subjects in attempting to predict when resumption will take place and when it will not. One implication, among others, would be the importance of relative difficulty within a task-series, and the studies of Ovsiankina (11) and of Pachauri (12) have shown that this is, indeed, a factor to be reckoned with.

If the whole story had been told, one might construct a satisfactory theory in terms of three variables: (1) personality organization (maturity and pride), (2) the nature of the task situation (importance or triviality with respect to the subject's view of his own status), and (3) difficulty of the task (with its effect on the subject's level of aspiration for the immediate future).

Unfortunately, at least three complications intervene. For practical purposes of prediction and understanding, one grave difficulty stems from the interdependence of the three variables. Level of aspiration, for example, may vary with age, personality, and the nature of the task situation (5). At our present stage of knowledge, this interdependence creates an indeterminacy only to be surmounted by discovering the precise functions which relate the major factors to one another.

A second difficulty inheres in the apparently contradictory nature of some

[1] 'Failure' in these experiments is defined in terms of the experimenter's disparaging comments on the time consumed by the subject. Most subjects *lowered* their preference-ratings for a particular task after 'failing' on it.

of the evidence. J. F. Brown (2) has reported studies in which his subjects carried on two types of task, one an informal set and the other a group of 'intelligence tests.' All tasks were interrupted and the subjects recalled significantly more of the 'intelligence test' tasks than of the informal ones. On the surface, this result contradicts the evidence of Rosenzweig (15). Numerous hypotheses may be invented to account for the discrepancy, but it is difficult to demonstrate the validity of any of them. Were Brown's subjects too immature or unambitious to recognize that failing to complete 'intelligence tests' meant a threat to their status? An unlikely hypothesis. Rosenzweig used separate groups of subjects where Brown used the same individuals, for whom he interspersed intelligence tests and informal tasks. Is it possible that in Brown's experiments tensions produced by interruption of tasks affected neighboring activity within the person, and so account for the discrepancy? We have no direct evidence. Rosenzweig used what was essentially the technique of Zeigarnik (half the tasks interrupted and half completed, and randomly distributed), while Brown interrupted all the tasks on which his subjects worked; here, too, may the degree of neighborhood among tension systems somehow account for the divergent reports? Again it is hard to say anything positive, though some evidence to be presented below may illuminate the last hypothesis to some degree.

One last difficulty with the treatment of success and failure grows out of our present inability to say what constitutes 'failure.' It is legitimate for some purposes simply to define 'failure' arbitrarily in terms of an experimental operation. Such is the procedure of Sears (18), Nowlis (10), Cartwright (3) *et al.*, and it has many advantages. It is patent, however, that such a definition omits the important dynamic characteristics of failure as an experience, and experimenters in this field will agree that no technique univocally produces a clear reaction of 'failure.' In fact, the reverse is true. All experimenters report deviants whose atypical behavior can be explained only by the assumption (sometimes verified by interview) that the situation produced no experience of failure (v. Cartwright above, p. 331). This very discouraging fact stems, of course, from complexity and will disappear only as we untangle interacting and interdependent components. But it also points a warning finger. If the reactions of different subjects are widely divergent, and if the effects of a particular experimental treatment may be known only from a post-experimental interview, investigators should beware of lumping subjects indiscriminately together for statistical purposes. The averaging of scores resulting from qualitatively different psychological situations may be misleading. The disentanglement of the basic factors may be postponed almost indefinitely unless more attention is paid to the individual case or to group studies in which the groups are chosen on some meaningful psychological basis. Some system must be devised for the conceptualization and measurement of the 'personality' factors that presently beshroud the psychological mechanism of failure.

3. *Homogeneity of interruption.*— Some experiments of the present author have raised still other questions concerning the fundamental dynamics of interruption. In connection with an investigation of relationships between retroactive inhibition and various motivating conditions (13), an attempt was made to compare a series of tasks under tension with a series which was presumably free from such influence. In accordance with a straightforward interpretation of Zeigarnik's hypothesis, an experiment

was designed in which fifteen completed tasks comprised one series (presumably without tension) and fifteen interrupted tasks constituted the other (presumably tense). The results challenge the traditional treatment of these matters, for consecutive interruption of all the tasks in a series appears to rob interruption of its characteristic tendency to produce heightened recall. A series of uniformly interrupted tasks fared no better than a set uniformly completed. This result appeared under conditions of instruction and other experimental factors which should be favorable for the standard Zeigarnik-effect, and which actually gave rise to that effect in control experiments using randomized interruption of one-half the tasks in each series. This discovery was preliminary and incidental to other studies, but if it should be confirmed in more thorough investigations, it will necessitate a re-evaluation of some of the concepts with which Zeigarnik and her followers have dealt.

Three tentative conclusions may be drawn. First, the development of tensions from interruption may be a relational affair determined by the presence of similar systems existing in a completed state. Second, persistent interruption may affect the attitude of the subject toward the entire experimental situation, in which case the results reported variously by Zeigarnik, Ovsiankina, Fajans (4), and Marrow (9) would lead us to expect observable changes in behavior. Third, the difference between the experiments of Rosenzweig and those of J. F. Brown, discussed above, may reside largely in the fact that Rosenzweig's technique compares systems under tension, while Brown is, perhaps, considering two systems that are not so affected, since *all* his tasks are interrupted.

4. *Retroactive inhibition of Zeigarnik's effect.*—One other finding deserves comment. Zeigarnik, in her original investigation, found certain cases in which the high ratio between the recall of interrupted tasks and the recall of completed ones was reduced by activities which followed the usual experimental procedure. When subjects were summoned to the telephone for an important call, only to discover that they had been tricked, the recall test failed to show the normal advantage for interrupted tasks. Similar results, varying in their effectiveness, were found when the subjects were sent to another room to participate in a different sort of experiment, or when they were engaged in a conversation about their own pressing problems. Such deviations from the basic technique were said by Zeigarnik to produce a change in the *Umfeld,* the surrounding conditions by which the tensions were determined, and consequently to dissipate the tensions. Conversation with subjects who continued to recall more interrupted than completed tasks despite the change revealed that they had considered the new situation only an interlude and that recall for them was easy. These subjects were said to be psychologically still 'in' the original task-situation.

In the course of the preliminary experiments described above, the present author studied the relationship between recall for interrupted tasks and recall for completed tasks in the standard retroactive inhibition set-up. Two groups of subjects were used. Group 1 worked on a series of sixteen tasks, half of which were interrupted and half completed. A five minute intermission followed, after which the subjects were told that the experiment would continue, and a second series of tasks (this time only eight, all completed) was presented. At the end of the eighth task, recall for the first set of tasks was tested. Group 2 followed exactly the same procedure except that no second series was given, and these subjects merely read an interesting book

for the period during which Group 1 worked on the second set of tasks.

In terms of Zeigarnik's hypothesis, it might be expected that the second group would experience the less continuity between the first and second parts of the experiment, since the work on another series of tasks was treated as a simple continuation of the original experiment and was apparently so accepted by the subjects. While it is possible that a similar attitude was present among the group with no interpolated series, any existing differences should, according to Zeigarnik's formulation, presumably be in the direction of maintaining the tension in those subjects who had the less psychological change between the two parts of the experiment. The reverse appears to be true. Table I gives the

TABLE I

	Mean I/C *
Group 1 ($N = 10$)	.904
Group 2 ($N = 10$)	1.396
Diff.	.492

$F = 7.86$

($P = .05$ when $F = 4.41$; $P = .01$ when $F = 8.28$)

* The fraction I/C is the ratio between the recall of interrupted tasks (I) and the recall of completed ones (C).

results for tasks recalled during the first three minutes. The magnitude of the difference when subjects were allowed indefinite time for recall was somewhat smaller, but the significance of that difference was nearly as high ($F = 7.0$).

Three hypotheses present themselves. In the first place, it may be true that any kind of activity tends to destroy 'tensions' more quickly when it is intense than when it is relatively mild. Such an effect, if it can be demonstrated, might account for at least some of the phenomena of retroactive inhibition. In the second place, the influence of similarity cannot be ignored. We know from many investigations that increased similarity between original and interpolated learning increases retroactive inhibition. It may be true that similarity also determines the degree to which tension systems may interact in such a way as to dissipate each other. One may even conceive that such a mechanism lies at the root of the classical retroactive inhibition phenomenon, if he assumes first that some sort of tension is always necessary for recall.[2] In the third place, the effect here demonstrated may arise from the tendency for completed tasks in the second series to serve as substitutes for the interrupted ones in the first set. Thus the original tension may have been released by success on later tasks. If the last explanation is the correct one, we need deal with no new concept, since the problem of substitution has received considerable attention. On the other hand, the data under consideration would necessitate a careful reëxamination of the criteria for substitute value. For nothing in the nature of Series 2 implies greater equivalence (and consequent resolution of tension) for its members than for the completed tasks within Series I itself. It seems difficult, in other words, to reconcile the two facts appearing in this study: (1) that some completed tasks are necessary before interruption becomes effective, and (2) that the addition of a few more completed tasks will begin to obliterate those effects. We may not, however, dismiss this possibility without further investigation, and psychological theory is now ready to profit by quantitative studies showing how differential recall changes with different proportions of completed and interrupted tasks.

5. *Discussion.*—Two principal weaknesses in our knowledge about the ef-

[2] For fuller consideration of this possibility, see Appendix D in W. C. H. Prentice, The effect of intent and interruption on retroactive inhibition, Ph.D. Thesis, Harvard Univ. Lib.

fects of interruption emerge from the present survey. The first has to do with the conceptualization and representation of the variables involved, and the second concerns the inadequacy of experimental methods.

The life space.—With respect to the former problem, it would seem that mere manipulations of the concept of tension are unable, without other considerations, to account for the complexity of the phenomena. It is quite possible, however, to use Lewin's conceptual framework in describing these facts, so long as we do not confine ourselves too strictly to the events in the 'inner personal regions.' In fact, the studies of Fajans (4) and of Marrow (9) have already pointed the way to a relatively simple solution.

Fajans has shown that differential reactions to failure may sometimes be explained in terms of changes in the subject's perception of the total situation, by changes, that is, in what Lewin has called 'cognitive structure.' This sort of change may take place, for example, when the experience of failure leads the subject to see certain reactions as obstacles rather than as the pathways to his goal they had previously seemed.

Marrow, in a careful and ingenious experiment, reversed the Zeigarnik-procedure. He instructed his subjects in such a way that interruption signified 'success' on the tasks, since they were to be allowed to continue only so long as the experimenter was not sure the method had been grasped. Under Marrow's conditions, the subjects recalled more completed than interrupted tasks. Thus one sees that tensions are not necessarily specific to particular tasks, but may concern instead the whole experimental situation, wherein the goal is probably something very general like 'pleasing the experimenter,' 'proving one's ability' or the like. In Marrow's cases, interruption was a sign of achievement and actually had the effect of *resolving* the tensions set up by the original intention or 'quasi-need.'

Probably all, or nearly all, of the experiments on interruption have involved a situation essentially like Marrow's, the true goal being more general than the completion of a simple task. Typically, the goal is one of pleasing the experimenter, though special instructions may introduce further complications, and the task simply provides the distinguished path to that goal. The force in the direction of completing the task is really a resultant force and need not imply any positive valence of the task itself.[3]

Of the several effects that interruption may have on cognitive structure, two are concerned with changes in the degree to which one path is distinguished from others. Unfortunately, neither Lewin (8) nor Leeper (7) deals adequately with the factors that determine which path will be the distinguished one. Lewin, in his preoccupation with motivational problems, defines the distinguished path in terms of maximum valences among the various means to a goal. It should be clear, however, that in many cases the choice of a particular path is largely a perceptual matter. Leeper has apparently grasped this fact, for he uses a perceptual example in at least one case (7, p 161), but the relationship is nowhere formalized. In the typical interruption-situation, the means to the goal will have its attractiveness determined by, among other things, the *apparent* degree of difficulty involved, and we must at some point concern ourselves with the dynamics of these appearances. The crucial need at the

[3] In some cases, of course, the task will possess a positive valence, *i.e.* will be intrinsically attractive. In such instances, 'interruption-tensions' may be expected to appear despite the absence of the conditions described in the following paragraphs.

moment, however, is to recognize that, in experiments of the Zeigarnik type, the task must be viewed as a path to a larger goal, that the task may change its path-character, and that any changes must result from identifiable factors in the total situation.

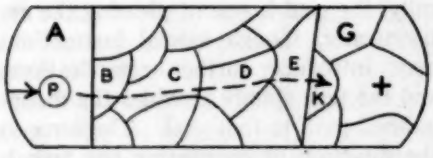

FIG. 1. $f^*_{A,G}$ in $w_{B,C,\cdots K}$ where K is a sub-region of the goal G. K stands for the solution or completion of the task, while the intermediate regions represent stages of activity in working on the task.[4]

Let Fig. 1 be taken to represent the psychological situation at the beginning of any particular task in an experiment on interruption. The various regions represent stages in the task's solution. It should then be clear, for example, that an interruption that takes place just before success would have been assured may make it clearer than ever to the subject that just this path is the proper one for him to follow, or, in other words, may make the path more clearly 'distinguished' (Fig. 2). When

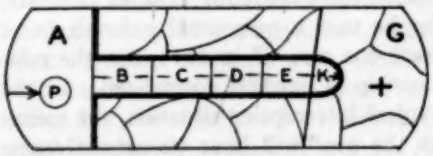

FIG. 2. The same resultant force shown in Fig. 1 will be stronger here since the more clearly distinguished path will lead to a diminution of the strength of forces along competing paths. The form of this diagram has been chosen as a means of representing a more stable cognitive structure.

[4] $f_{A,G}$ = force in region A toward region G. f^* = resultant force. $w_{B,C}$ = path (from German *Weg*) from region B to region C. + = a positive valence or attraction to the region representing a particular act. − = a negative valence or repulsion.

it is not possible for the subject to be sure of his ability to follow the path to its end, however, he may come to see the formerly distinguished path as a cul-de-sac, with the result that the continued presence of his general need to please the experimenter will nevertheless be insufficient to direct him toward resumption of the specific task on which he was interrupted, but may, on the other hand, lead him to seek actively for another path to the same goal. In such cases, the former path may be described as less well distinguished, and the resultant force along its direction may show a consequent decrease in strength. Such a situation is represented in Fig. 3 and

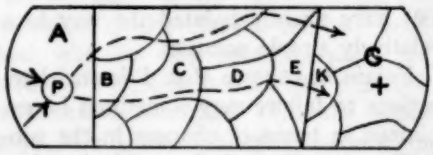

FIG. 3. Two resultant forces toward the general goal may be nearly equal as a result of relatively unclear structure. The path representing work on assigned tasks ($w_{B,C,\cdots K}$) may have strong competition.

might be verbalized by the subject somewhat as follows: "I thought that was the way to satisfy the instruction, but now I am not so sure; maybe there is a better way."

Other changes may take place in the cognitive structure. When the attempt to complete a task has led to unpleasantness and feelings of insecurity, that activity may take on a negative valence (Fig. 4) or may (more likely) take on

FIG. 4. Some or all of the part regions of a particular path may acquire a negative valence with the consequence that $f^*_{A,G}$ may, in the absence of any alternative path, be opposed by strong forces.

FIG. 5. The structure represented here is that of a subject who views $w_{B,C,\ldots K}$ as bifurcated and as having two possible ends, the choice being out of the subject's control. The region corresponding to the first step of the task has, consequently, both a positive and a negative valence. (The wavy arrow shaft indicates the inability to retrace one's path, v. Leeper, 7.)

the character of a bifurcated pathway which could lead to success, but might lead also to failure (Fig. 5). In either case, the resultant force in the direction of resumption of that particular task would be weakened. This state of affairs may characterize the children in Rosenzweig's experiments who tend to resume those tasks on which they have 'succeeded.' It may also account in part for the conditions under which 'repression' takes place, though the latter is certainly more complicated. In the case of other subjects, however, failure may be experienced (for reasons which we cannot now specify) as diffusing the region in which the person finds himself, so that it is the immediate situation in which he feels insecure as a result of not having reached his goal. Thus the present state acquires a negative valence, and the force away from that state is enhanced (Fig. 6). Such a situation may be representative of the subjects in several experiments who show a 'need for success.' What Rosenzweig calls

FIG. 6. Region *A* has acquired a negative valence, and the resultant force in the direction of *G* along $w_{B,C,\ldots K}$ is thereby enhanced in strength.

'strong ego-defenses' may, moreover, be correlated with a tendency toward this diffusion of the effects of failure.

Finally, a long series of interruptions may lead the subject to see his path as considerably shortened. He may, that is, so 'restructure' the situation that he perceives the tasks as ones which he is not expected to complete and upon the completion of which his ability to please the experimenter does not depend. He might thus conclude that his goal could be reached merely by attempting the task, and interruption would leave no tension (Fig. 7). Evidence reported

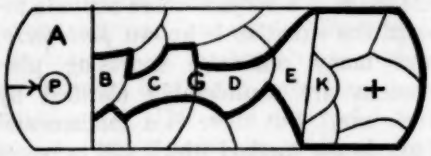

FIG. 7. In this drawing, all the sub-regions of $w_{B,C,\ldots K}$ have become incorporated in the goal region, with the consequence that activity corresponding to any of them is equivalent to reaching the goal. A similar situation might exist with respect only to some of the stages of the performance of a task, but the principle would be the same. In either case, some degree of activity short of completion would resolve the tensions arising from the original instruction.

above indicates that something of the sort may take place when series of tasks are homogeneously interrupted. Superficial observation of the subjects' behavior seemed to show a marked lessening of tension in many cases after several interruptions, a change that may arise from the kind of restructuring shown in Fig. 7.

Some issues remain. The apparent reduction of tension by later activity (even of the same sort) does not lend itself to easy conceptualization. Further research will certainly be necessary before we can identify the effective differentiæ between the conditions of Zeigarnik and those in the experiment of the writer reported above. More trou-

blesome still is the question of whether the forces toward recall and those toward resumption may be different. One may suggest tentatively that the tendencies to act and to remember take place at different levels of the life space, and the problems of 'repression' may also have to be dealt with in this fashion. It is, however, too soon to say whether such representation can be consistent and useful.

With the exception of such cases, it is apparently possible, by using these conceptualizations individually or in some combination, to represent adequately the life space of a subject whose attitude toward the situation is known *post facto*, and many otherwise confusing phenomena are considerably clarified by their use. But there is a fundamental fault in our method which still prevents truly satisfactory treatment of the complex issues involved in the psychology of interruption. We have not yet established the relations between experimental variables and our conceptual diagrams.

The relation of objective variables to the life space.—How is it possible to predict in advance which of the various diagrammatic representations will actually apply to a particular subject or to a particular experimental arrangement? What, in fact, are the experimental variables known to affect the response to interruption? In tabular form, they may be ordered as follows:

Factor	Investigator
Structure of the task	Zeigarnik, Ovsiankina
Age of subject	Zeigarnik, Fajans, Rosenzweig
Fatigue	Zeigarnik, Pachauri
Time spent on tasks	Pachauri, Prentice [5]
Difficulty of tasks	Ovsiankina, Pachauri
'Personality' of subject	Ovsiankina, Rosenzweig
'Importance' of task	Brown, Rosenzweig
Success vs. failure	Rosenzweig, Nowlis, Cartwright
Attitude of subject	Zeigarnik, Ovsiankina, Marrow
Nature of post-experimental activity	Zeigarnik, Prentice
Homogeneity of interruption	Prentice

[5] Dissertation cited above.

A mere glance at this tabulation is sufficient to remove any wonder at the fact that many investigators (see especially Boguslavsky and Guthrie, 1) have been unable to reproduce '*the* Zeigarnik effect.' Wonder should be reserved, on the contrary, for the fact that so little effort has been expended in constructing a uniform and thoroughgoing theory of the processes involved.

It must be apparent that there is something fundamentally wrong with our approach to these issues. All our effort has been directed toward a conceptualization of the psychological variables that intervene between the objectively controllable situation and the subject's reaction; practically no effort has been used in trying to establish the relations between the objective situation and the intervening variables.

In a recent paper, Spence (19) has made essentially the same point in criticism of the Lewinian position. Spence, however, errs in his failure to recognize the importance of the phenomenological method in understanding the nature of the intervening variables. His position is an evidence of the latent dualism which is seemingly ineradicable from the stimulus-response psychologies. The behavioristic viewpoint and tradition apparently creates a blind-spot for the perfectly obvious fact that phenomena, after all, *are* intervening variables. Only an implicit acceptance of the notion that experiences are of a certain

'mind stuff' incapable of operating in a materialistic world, or that they are, at best, a sort of epiphenomenon, can account for Spence's rejection of phenomena from the realm of scientific data. The only serious troubles with phenomena are (1) that we may get only crude approximations from the subject and (2) that they do not exhaust the stock of intervening variables.

On the other hand, Spence, like Hull (6), correctly and forcefully insists that the nature of theoretical constructs in psychology demands the establishment of functional relations of two sorts, those relating manipulable variables to constructed intervening variables, and those relating the latter to measurable reactions. It is the former task which has been neglected in dealing with the problems of interruption. While a good case may be made for the order of procedure that has been followed to date, the time has now arrived when we need careful investigations of the functional interrelations among the experimental variables and of their relations to constructs like 'distinguished path' and 'valence.' Any such program will necessitate quantification of measures which have been so far only crudely rated with respect to strength. The difficulties involved in finding mathematical units expressing degrees of fatigue, of task-difficulty, of the subject's attitude toward the experimenter, and the like are truly staggering ones. It is such difficulties that have undoubtedly been responsible for two noteworthy phenomena of current psychological theory: first, the fact that the workers who insist on quantification uniformly work with peripheral and segmental problems (and usually with animals) and second, that psychologists who are impatient with our ignorance of central problems have consistently ignored the vital step involving connections between manipulable variables and the intervening constructs. But the issue cannot be postponed indefinitely.

6. *Summary and conclusions.*—Without any attempt at an exhaustive survey of the literature on the subject of interruption, it has been possible to show that certain confusions and contradictions have arisen since Zeigarnik's original contribution was published and that theoretical treatments have failed to keep pace with the evidence.

Two types of remedy have been proposed. (1) More attention should be given to the *cognitive* changes involved in many of the experiments, since treatment in terms of motivational variables alone has proved inadequate. Some specific examples of the types of cognitive restructuring that occur have been presented. (2) The need is crucial for studies that explore the relations between objective variables and the intervening variables represented by Lewin's life space. Many of the studies will be extraordinarily complex as a result of the large number of factors demonstrably involved, and such studies must consequently be rather precisely quantified. Until both these remedies have been adopted, the 'explanations' of the findings of Zeigarnik, Ovsiankina, Rosenzweig, Marrow and their various followers will remain speculative and scientifically unsatisfactory.

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AMOUNT OF REINFORCEMENT AND LEVEL OF PERFORMANCE *

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I. Introduction

The effects upon performance of quantitative variation in amount of reinforcement is a realm of motivation theory which, for unaccountable reasons, has been a stepchild compared to the considerable body of research upon the parallel problem of the effects of variation in drive. Treatises upon behavior (6, 7, 8, 10, 11, 13, 14, 17) have naturally been forced to raise the problem as no presentation of motivation could otherwise be presumed to be complete. More often than not, however, because of the scarcity of data, they have relied upon the results of an early study by Grindley (4) as definitive despite the fact that, as shall be shown, it is characterized by a number of serious methodological weaknesses. The most recent systematic treatment of behavior theory, the *Principles of behavior* by Clark L. Hull, is not entirely an exception to this rule. Though two later researches are utilized, one by Gantt (3) and one by Wolfe and Kaplon (16), which bear indirectly upon the problem, Grindley's results are still given heavy weight. These additional studies give Hull a somewhat broader base for theorizing than his predecessors, but he is still led to some generalizations which, owing to the meagerness of the data, are rather more speculation than inference.

The object of the present paper is to subject Hull's treatment of quantitative variation of incentive and performance to criticism and amplification in the light of a recent series of experiments by the writer (2). On the basis of this appraisal the writer will be led to reject Hull's strength-of-conditioning explanation of the effects of incentive variation upon action. Suggested in its stead will be a theory grounded upon emotional drive.

Before bringing the present writer's findings to bear upon Hull's analysis, it is advantageous to present to the reader some of the general shortcomings of the investigations from which Hull has largely drawn his facts.

II. Criticisms of Procedures

1. *Drive control.*—In inquiries concerned with the effects of different amounts of incentive upon behavior obviously drive, the internal complementary aspect of motivation, must be held constant. Variations of performance cannot be ascribed to variations in amount of food incentive, for example, if hunger drive is varying simultaneously. Certainly there must be a drive, for incentives can only be defined as objects appropriate to the satisfaction of a drive, but the amount of drive should not be a variable. On the grounds of this requirement Hull's key sources of information—the studies of Grindley (4), Gantt (3), and Wolfe and Kaplon (16)—are all questionable. In Grindley's study of chickens with rice as the variable incentive, there was no apparent control of amount of rice eaten by the various incentive-amount groups. All that Grindley says is that "when they had been tested they were given a large meal of rice. At other feeding times they had no rice" (4, p. 175). Further, there was no assurance that the time of arousal of hunger-contractions

* A critique of Hull's treatment with an alternative theory of incentive motivation.

was controlled.[1] The statement appears that "the experiments were always made at a time when the chicks were hungry" (4, p. 175) which is an interpretation, not a statement of conditions.

Wolfe and Kaplon (16), in their experiment with chickens receiving different amounts of popcorn as incentives, had a trial regimen of five successive trials twice a day. Such a procedure destroys equalization of drive. If various groups of animals receive different amounts of incentive upon their initial trial, upon any subsequent trial the assumption can no longer be made that drive is constant among the groups. Performance after the initial trial would be a function not only of incentive variation but of drive variation. Gantt (3) also had several trials a day in his study of dogs with food as the variable incentive. So he too may be open to the same objections.

2. *Appetitive range.*—It is reasonable to assert that before any overall generalizations can be made about the influence of amount of incentive upon level of performance the amounts investigated must cover the major portion of the appetitive range[2] of the animal investigated. With respect to this reasonable requirement Hull's sources of information are seriously weak. Grindley's spread of incentive-amount variation was from one to six grains of rice—certainly a minute per cent of the appetitive range even for a chick. Wolfe and Kaplon used the even smaller spread of one quarter to one grain of popcorn. Gantt's study was somewhat better in this respect, varying from one-half gram to twelve grams of food. But the latter amount is still not a substantial fraction of the appetitive range of a dog.

3. *Statistical rigor.*—The last basis of criticism is statistical. Hull's sources of information have some serious deficiencies in this regard. In Grindley's experiment there was no statistical analysis of the results, and with his observation that there were 'wide individual differences in behavior' even Grindley thought that the experiment was not sufficiently accurate to establish the results with certainty. Grindley's sampling procedure was also dubious. Thirteen chicks or over 25 per cent of the group were thrown out because "they were found to develop a dislike for rice during the experiments." The writer will contend later that by this procedure he missed an important fact.

Wolfe and Kaplon's experiment had no major statistical shortcomings. But their only reliable conclusion important for us is that four one-quarter pieces of popcorn produce higher levels of performance than one one-quarter piece. This fact does not take one far; certainly one point allows no curve to be drawn relating amount of incentive to level of performance.

The only published record of Gantt's experiment is a one-page note (3). Hull bases some of his interpretations upon a personal communication. These circumstances make the study difficult to evaluate. Hull reports, however, nothing about the statistical significance of the points to which a curve is fitted. The curve represents data upon *one selected* dog, selected because not all of the animals showed any consistent success in the situation. One selected dog, need it be pointed out, is a precarious basis for generalization.

In the series of experiments by the writer careful attention was devoted to the avoidance of the statistical and methodological weaknesses discussed

[1] According to Bousfield and Elliot (1) hunger drive is probably more dependent upon the occurrence of hunger contractions than simply upon time of deprivation.

[2] This term will be employed to denote amount of incentive, *e.g.*, food, which is just sufficient to maintain a particular animal for one day.

above. Drive was equalized for the groups of rats obtaining different amounts of incentive by feeding them individually, after the experimental run, up to a constant amount (varying with body weight), including the amount of food obtained in the experimental run. This procedure left each rat just before his test run with a 22-hour deprivation from a weight maintenance ration. The period of occurrence of hunger contractions was not left to chance, but was conditioned to the time of experimentation. To prevent intertrial variation of drive each rat was limited to one trial per day.

The amounts of food incentive that the different groups of rats received varied from one unit (1/50th of a gram of Purina biscuit) to 256 units (5 grams plus). This variation in terms of the average maintenance diet of the rats encompassed about 70 per cent of their appetitive range.

The experiments were so designed that, through analysis of variance, rigorous statistical evaluation could be made of all results. The initial exploratory experiment revealed that the effects of amount of incentive upon performance were so substantial that small sample analysis was indicated as the most efficient approach.

In the following appraisal of Hull's treatment of the problem of amount of incentive or reinforcement it should be remembered that whatever specific criticisms are made of the studies Hull draws upon are in addition to the general faults which have been described.

III. Amount of Reinforcement and Level of Performance

Hull bases his views of this problem primarily upon two investigations. The first, that of Gantt (3) on dogs, yields a curve relating *amount of food reward* used at each reinforcement of four conditioned responses to as many different stimuli, to the final mean *amount of salivary secretion* evoked by each stimulus at the limit of training. Hull stresses that this function (5, p. 125) is not to be confused with a learning curve; each point represents the final horizontal portion or asymptote of a separate and distinct curve of learning. From the second study, that of Grindley (4), Hull gives a curve (5, p. 126) he has fitted to data relating *speed in a runway* to *number of grains of rice* received as reward by the chickens employed in the study.

The relationship between amount of reinforcement and level of performance Hull envisages from Gantt's and Grindley's data to be a simple positive growth function,[3] such that "rate of learning [or level of performance as the writer puts it for reasons that shall be later discussed] is an increasing monotonic function of the amount of the agent employed at each reinforcement" (5, p. 127). In some critical respects the writer's findings do not bear out Hull's conclusion.

Figure 1 depicts the curve the writer obtained, as an average of three experiments, relating level of performance to amount of incentive. How does it compare with the Hull-Grindley curve?[4]

[3] This is a function of the type Hull now proposes as characterizing all learning curves, namely, $\frac{N}{S}H_R = Me^{iN}$ where ${}_sH_R$ is habit strength, M is the physiological maximum of habit strength, N is the number of repetitions of the reinforcement, and $i = \log \frac{1}{1-F}$ where F is a constant fraction of the difference between the physiological maximum of habit strength (M) and the habit strength immediately preceding the reinforcement.

[4] Conveniently for this comparison the measure of level of performance for both the writer's and Grindley's experiment was speed of locomotion down a runway, and the units of incentive amount were probably roughly similar in terms of the per cent of the appetitive range of the animals involved, grains of rice for Grindley's chicks, 1/50th grams of Purina for the writer's rat.

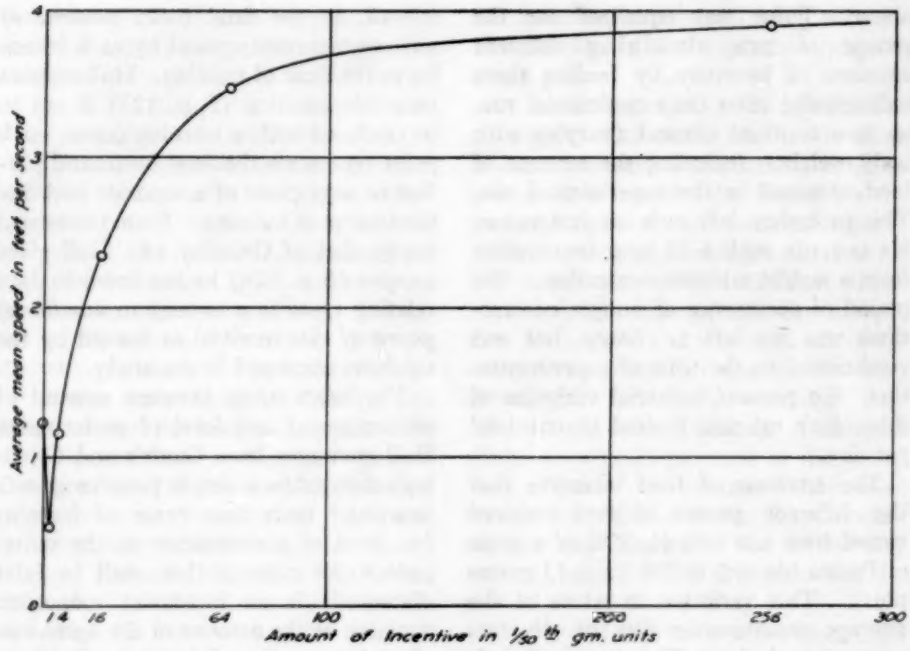

FIG. 1. Relationship between amount of incentive and level of performance at the limit of practice.

This curve is a mean outcome of three experiments. Each point, including that at zero incentive, represents the post-learning level of performance manifested by groups of rats receiving the amounts of incentive indicated.

It was the writer's original intention to place the Hull-Grindley curve, after differences of size had been compensated for, side by side with his own on Fig. 1 for comparison of shape. But on the scale necessary to properly represent his results it seemed of little value to diagram the Hull-Grindley curve since it became simply an almost vertical line pushed up against the *Y* axis which, at the limit of its rise, turned over sharply to a presumed asymptote. This latter curve is grossly different from Hull's picture of the function. Why does such a difference arise when the range of the amount variable on the abscissa is made to comprise a substantial portion of the appetitive range instead of just six grains of rice?

The difference occurs because the Hull-Grindley curve would have us believe that the increase in level of performance with increase in amount of reinforcement approaches an asymptote, *i.e.*, may not go any higher, within a food amount range of less than *six grains of rice*. In other words, within but a scratch on the surface of the appetitive range of the chicken, food amount, it is contended, has already exerted its greatest possible effect upon level of performance. The writer's results completely controvert such an unlikely state of affairs. His curve, disregarding for the moment the low end reversal, does appear to be a function of the simple growth type as Hull surmised, but the rate of increase, or *F* in Hull's terminology, is far smaller than would be implied by Grindley's data. These results, the writer feels, are more credible in that they show that amount of in-

centive progressively increases level of performance over some *70 per cent of the appetitive range* of the animal before the gains begin to approach a maximum. If the increments of level of performance approached an asymptote as fast as Hull seems to believe, there would be no difference in level of performance for almost the entire appetitive range. And it would be letting the tail wag the dog to conclude, as does Hull, that "the rate of learning (level of performance) is an increasing monotonic function of the amount of the agent employed at each reinforcement" (5, p. 127).

It is likely that Hull has been led to such an improbably early limit in his graph of the relationship between amount of reinforcement and level of performance because of the following two reasons: (*a*) The minute range of amounts of reinforcement (one to six grains of rice) and the unreliable points in Grindley's experiment obscured the fact that with larger amounts of reinforcement the gains in level of performance have every likelihood of going much higher. To observe the unreliability of the points one has only to turn to Grindley's learning curves (4, p. 174), from which Hull fitted his growth function. The curves taken as a group are not very far apart; they crisscross again and again, and Grindley appends to his own generalization, "But there were wide individual differences in behavior" (4, p. 175). Hull himself admits that the deviation of Grindley's points from the growth function he fitted to them are 'considerable.' A small change in the position of a point or two might have totally altered the picture. (*b*) Unlike Gantt's curve (to be discussed shortly) which related level of performance to amount of reinforcement only after the changes in learning for the animals receiving each amount of reward were complete, Hull's curve from Grindley's data was based upon an average of the responses of the animals to the different amounts of food from the *third to the seventh trial.* This period, as an examination of Grindley's learning curves indicates (4, p. 174), was at the very *outset* of learning when learning changes were largest, and when, as has been said, the performance of each of the groups going to the different incentive amounts was still highly variable (as can be seen from the crisscrossing of the learning curves). But apart from the general dangers of fitting a function to such variable and unreliable data, the writer has found in his learning curves that it often takes more than three trials to allow the differential in reinforcement full opportunity to get in its effect upon level of performance. If a cross-section of the learning curves for different amounts of reinforcement is taken at such an early stage of learning, the gain in level of performance with increased amounts of reinforcement is likely to be clouded by the fact that the animal has not yet had sufficient experience with the situation to develop a clear appreciation of the size of the reward he may reliably expect.

Let us turn now to consider Gantt's curve which Hull cites as his second source of evidence as to the relationship between level of performance and amount of reinforcement. These data, it will be seen, do not compel the writer to alter his contention based on the comparison of his results with Grindley's that the gains in level of performance, in disagreement with Hull, approach an asymptotic limit not early but late in the appetitive range of the animal concerned.

Gantt's measure of performance was quite different from speed in a runway; it was rather the amount of conditioned salivary secretion given by a dog to four stimuli respectively reinforced with one-half gram, one gram, two grams, and

twelve grams of food. A simple growth function, it would appear, describes the relationship between the two variables. The level of performance as the function is fitted by Gantt seems to be approaching an asymptotic limit at approximately the 12-gram amount of reinforcement. As 12 grams of food to a dog is in all probability but little greater a percentage of his appetitive range than is six grains of rice to a chick, Gantt, like Grindley, appears to find, contrary to the writer's results, that the effect of amount of reinforcement upon level of performance has its maximum at almost the very outset of the appetitive range. It would be a mistake, however, the writer feels, to take this result too seriously. For the whole argument for the function turning over toward an asymptote between two and twelve grams hinges upon one point from one selected dog. Certainly more evidence than this is needed for generalization. Further, it is worth repeating here, from the general criticisms of Gantt's experiment, that there was no assurance of adequate control of drive. Since Gantt used several trials per day it is quite possible that variation in drive entered to distort his results.

Finally, there is, of course, the possibility that amount of reinforcement affects a performance like conditioned secretion quite differently than it does one like runway speed. But with Gantt's results so insecurely established it would be premature to take this easy tack as an explanation of the differences between Gantt's findings and those of the writer.

Having now concluded the criticisms of Hull's placement of an asymptotic limit so early in his growth function, let us now turn to another feature of Fig. 1. In this curve, from the zero amount of reinforcement to the four unit amount, a dip in level of performance can be observed with a minimum at one unit reward. If this minute-incentive dip can be substantiated, it forces another modification of Hull's conception of the relationship of level of performance to amount of reinforcement.

The results as shown in Fig. 1 in the region of zero incentive up to 16 units incentive are composed of the average of two experiments. In Experiment I a different group of rats was *started directly* at each of the various incentive-amounts and, after learning changes had ceased, the level of performance was ascertained for each amount group. In Experiment II all the rats were started at 16 units incentive and then when their performance had stabilized (learning changes ceased), separate groups were *shifted down* to zero, one, and four units incentive, at which amounts, when the variability introduced by the change had diminished, the associated levels of performance were ascertained.

With these considerations in mind, the evidence can now be presented for the conclusion, pictured in Fig. 1, that rats run faster with a complete absence of incentive than with a very small incentive. The first source of evidence is quantitative, the comparison of the levels of performance, in Experiment II, of the animals receiving no food incentive with those receiving a minute food incentive (one unit). The no-incentive group, it was found, *exhibited a higher level of performance* significant at better than the three per cent level ($t = 2.33$).

A second source of evidence lies in a series of qualitative observations which revealed that the receipt of a minute amount of incentive is productive of a number of behavior patterns indicative of frustration. The qualitative behavior patterns observed involved the following components: peering, scurrying, jerking of the head and body, biting of objects, jumping, face-washing, grooming, scratching of the body, prolonged smelling, and fitful retracting. The appear-

ance of frustration in the various small incentive groups was distributed as follows: (1) The unit incentive group of Experiment I, and to a smaller extent the four-unit incentive group, began to manifest the frustration symptoms around the twelfth daily trial. From that time to the end of the experiment the symptoms became more pronounced in form and general in the group. (2) In Experiment II, about four trials after the shift from 16 units incentive, these same symptoms began to appear in the unit incentive group and to a smaller extent in the four unit group, growing more pronounced in form and general in the groups to the end. The prevalence and amount of these behaviors, particularly of the more violent kinds, as jumping to get out of the apparatus near the food box and biting the apparatus, were appreciably greater in the one and four unit incentive groups of Experiment II than in the corresponding groups in Experiment I. (3) In the group shifted in Experiment II from 16 units incentive to none, there was hardly any appearance of the frustration symptoms, particularly of the more violent forms (2, p. 497).

These findings, both quantitative and qualitative, suggest that there is a frustration effect specifically associated with receipt of minute incentive amounts, particularly one unit and to a lesser extent four units, which is not associated with the receipt of large incentives nor *with the complete absence of incentive.* The very few occurrences of the frustration symptoms in the group receiving no incentive are adequately accounted for on other grounds, in terms of the procedure of Experiment II. This experiment, it has been pointed out, unlike Experiment I, involved a shift from 16 units incentive downward to zero, one, and four units. This *downward contrast* produces on its own account, as will be discussed later in this paper, some frustration and some depression of performance—a 'depression effect.'[5]

If it is granted that there is a frustration factor associated with the receipt of a very small incentive which is not associated with the complete absence of an incentive, can it be explained? A reasonable hypothesis is as follows: Obtaining a minute amount of food (*a*) arouses anticipatory emotional tensions without allowing any appreciable resolution, and (*b*) serves to remind the animal of his hunger, thus possibly stimulating his hunger drive without, however, improving his chances for obtaining more food. Hence, after the rat obtains the small incentive, he must remain in a state of heightened tension for an (to him) indefinite period (it will be remembered that the rats were fed an appreciable and variable amount after their daily test run was over). This state of heightened tension is unpleasant. Therefore, though food is present and the animal has had no food for some twenty-two hours, the situation is labeled 'frustrating.'

If a frustration effect which dips the level of performance for minute incentive amounts below that associated with complete absence of incentive is so well established on both quantitative and qualitative grounds, why does not the dip appear in the Hull-Grindley curve?

[5] That this 'depression effect' was present in Experiment II in addition to the direct frustration effect of minute incentives is supported by two considerations. First, the number of frustration symptoms in the one and four unit incentive groups of Experiment II was substantially greater than in Experiment I, where only the direct frustration of minute incentives was operating. Secondly, though the rats of Experiment II were natively faster (because younger) than those of Experiment I, as revealed by a significant difference in speed under the same conditions at the 16 unit incentive level ($t = 2.26$), their speed advantage, presumably because of the depression effect of downward contrast, was lost after their shift from 16 units incentive down to four or one.

The answer is, in all probability, that Grindley simply discarded the effect. To quote the previous remarks of the writer on this subject (2, p. 498): "In Grindley's study the significant statement is found that 'some of the chicks were found to develop a dislike for rice during the experiments.' Thirteen chicks or more than 25 per cent of the entire group were discarded because of this dislike. It is extremely likely that these chicks were only expressing the behavioral results of frustration upon receiving such small incentives (one, two, four, or six grains of rice). And Grindley, instead of studying this phenomenon on its own account, threw these chicks out of the investigation." With such a likely explanation it is hardly necessary to suggest as an alternative that Grindley with only seven learning trials might have given the frustration effect insufficient time to appear. It will be remembered that in the writer's investigation with rats the effect did not appear until the twelfth trial. Wolfe and Kaplon in their check upon some portions of Grindley's findings used five successive trials twice a day. So frustration, derived as the writer explained it, could hardly have occurred.

Since the effect of zero incentive is covered in Gantt's curve by extrapolation instead of investigation, and since so little is available about the details of the experiment, it is hardly profitable to discuss it in this context. We know Gantt used more than one trial per day, however, so it would seem that a frustration effect would be thus precluded.

On the basis of the consideration advanced in this discussion the writer would tie to Hull's 'increasing monotonic function,' as a picture of the relationship between level of performance and amount of reinforcement, a minute reward reversal. The turn is due to the additional variable that enters at the low end of the amount of reinforcement scale. Hence the overall function cannot be characterized as monotonic, since with increase in the independent variable—amount of reinforcement—the dependent variable—level of performance —may show decrease as well as increase.

To sum up this section of the paper the writer would suggest as emendations to Hull's generalization that level of performance "is an increasing monotonic function of the amount of the agent employed at each reinforcement" (5, p. 127), (*a*) associated with the receipt of minute amounts of incentive there is a dip of level of performance below that characterizing larger incentives or complete absence of incentive,[6] and (*b*) beyond this minute incentive region, level of performance, when learning changes have levelled out, is an increasing function approaching asymptotically to a limit not at the outset but deep in the appetitive range of the animal concerned.

IV. Amount of Reinforcement and Number of Reinforcements

Let us now consider the effects of amount of reinforcement upon the *learning curves* as evidenced by groups of animals running to different amounts of incentive. As has been previously stated, Hull's fundamental assumption (5) is that all learning curves will be simple growth functions of the number of reinforcements (N), mathematically of the form $M-Me^{iN}$ where M is the asymptotic limit of growth or learning, and $i = \log \frac{1}{1-F}$ where F is a con-

[6] By complete absence of incentive is meant of course absence of the material which is presumed to be the major interest of the animal at the moment, *i.e.*, in the writer's experiments, food. It must not imply that there might not be other minor incentives present as, for example, escape from the situation.

stant fraction of the difference between the physiological maximum of habit strength (M) at the particular amount of reinforcement and the habit strength immediately preceding reinforcement. Cast into Hull's terminology the problem of this section becomes: What effect does amount of reinforcement have upon the parameters M and F of the learning function? The answer to this question, of course, can only be obtained from systematic learning curves under different amounts of reinforcement. Regretting the fact that "unfortunately no such investigation has yet been reported in detail" (5, p. 128), Hull offers a few surmises based upon Gantt's and Wolfe and Kaplon's data. He suggests as a hypothesis that an increase in the quantity of a reinforcing agent increases the rate of learning by raising the limit (M) to which the curve of habit strength approaches as an asymptote. The rate of approach (F) to this limit, Hull suggests, possibly remains constant for different amounts of the reinforcing agent.

An investigation relative to which this hypothesis can be tested has been reported in detail by the writer (2). The learning curves are reproduced in Fig. 2. One would only have to smooth these curves to see that they approximate Hull's hypothesis as to what would ob-

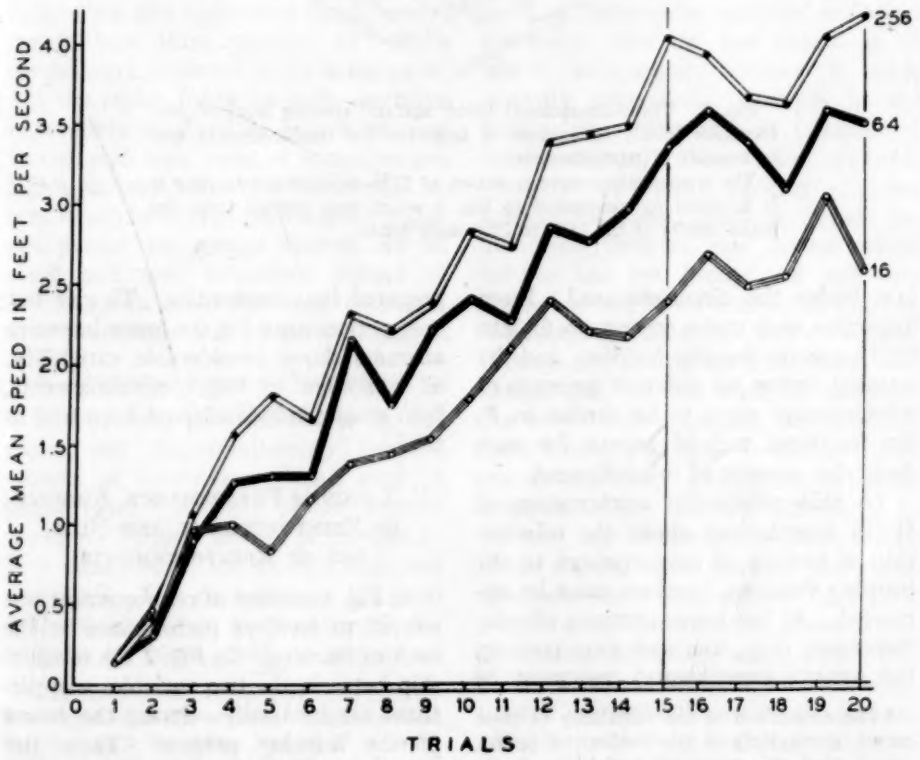

FIG. 2. Relationship between number of reinforcements and level of performance for different amounts of incentive.

These are the learning curves manifested by groups of rats receiving the amount of incentive indicated [reproduced by permission from (2, p. 488)].

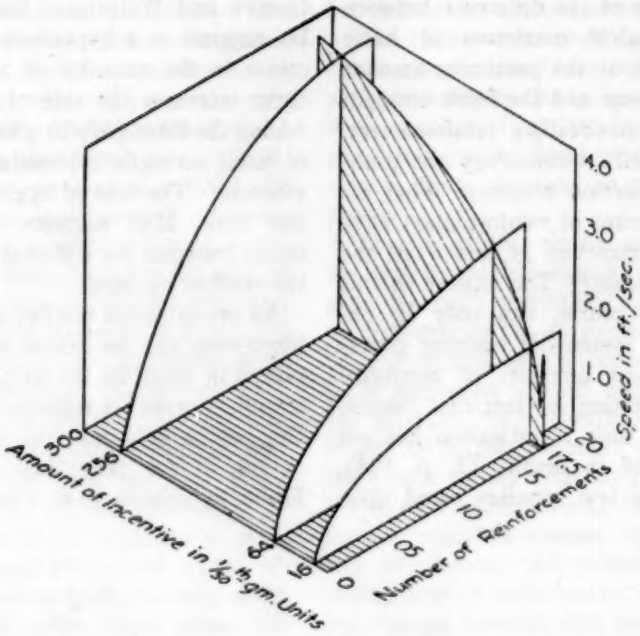

FIG. 3. Three-dimensional fitted 'surface' relating level of performance jointly to amount of incentive (or reinforcement) and to number of reinforcements.

The cross-section curve is drawn at 17½ reinforcements since it is intended to correspond to Fig. 1 which was plotted from the mean results of the 15th to 20th daily trials.

tain under the circumstances.[7] From inspection each curve appears to fit into Hull's generic learning function, and the learning curves for different amounts of reinforcement seem to be similar in *F*, the fractional rate of growth for each particular amount of reinforcement.

To this substantial confirmation of Hull's expectations about the relationship of amount of reinforcement to the learning function, cautions must be appended. At the lower amounts of reinforcement (*e.g.*, one and four units in the writer's experiments) one must be prepared for complexities. To give but one, performance for the lower incentive amounts shows considerable variability, as compared to larger reinforcements, both among individuals and from trial to trial.

[7] The correlation of the variability in these curves, particularly in the leveling off period, reveals that the causes of variability are in large part extraneous, affecting the three curves similarly, rather than intrinsic to differences in amount of incentive. Hence smoothing is a fair procedure.

V. LEVEL OF PERFORMANCE, AMOUNT OF REINFORCEMENT, AND NUMBER OF REINFORCEMENTS

In Fig. 1 amount of reinforcement was related to level of performance at the *limit* of learning. In Fig. 2 the relationship between the two variables was pictured longitudinally—during the *course* of the learning process. These two graphs can be tied together into one integrated view by a three-dimensional representation. This surface appears as Fig. 3. For greater clarity and sim-

plicity the constituent points of Figs. 1 and 2 are not represented, only smoothed curves. The longitudinal learning curves for different amounts of reinforcement, and the cross-sectional view of the relationship between amount of reinforcement and level of performance,[8] when learning changes have largely reached an end, can be clearly seen here in their interrelationships. The cross-section curve through the learning curve maxima is itself approaching an asymptotic limit, but *deep* in the appetitive range. The cross-section curve is a growth function except for a low end complexity—a minute incentive reversal.

This surface, the writer believes, is a good first approximation to the interrelationships among the three variables. It is only a *first* approximation, however, for at least three reasons: (1) More overall work is needed to fill in the gaps; (2) the region below 16 units incentive is certainly idealized as groups running to one and four units of incentive are highly variable, and (3) the minute incentive dip in level of performance might not persist for groups started out at small and zero incentives instead of shifted down after they had stabilized their performance at 16 units incentive.

VI. LEARNING VERSUS PERFORMANCE

The opening problem raised in this paper was the relationship between amount of reinforcement and level of performance. Implicit in the writer's use of the phrase, 'level of performance,' is a disagreement of conceptualization with Hull. This disagreement has been deliberately held in abeyance until differences in facts could be reconciled. Where the writer has generally used the term level of *performance* in denoting the dependent variable, Hull would speak in terms of level or rate of *learning*. The different levels of activity (runway speed in Grindley's and in the writer's experiments) associated, at the limit of practice, with different amounts of reinforcement Hull conceives of as differences in learning or *habit strength* (sH_R in Hull's notation). Hull metricises this learning variable by defining one hab (short for habit) to be one per cent of the physiological maximum (M) or 'total possibility of learning' under optimal conditions.

The writer cannot object to Hull's scientific procedure in inferring from observable speed of response effects upon an 'intervening variable' or logical construct. But he can object as to which intervening variable is most properly concerned. Changes in the longitudinal curve with N (number of reinforcements), the writer agrees, properly represent learning—growing mastery of the situation. And the difference between two curves where neither has yet leveled off and one group has had fewer trials than another is a learning difference. But differences between the two curves when both have leveled out, 'at the limit of practice,' the writer insists, are not differences in habit strength as Hull would say, they are substantially differences in *drive* strength. They are not differences in *learning*, but differences in *performance* which depend upon learning and drive determination. Hull defines (M) as a physiological limit of habit strength under optimal conditions which equals 100 habs. The limit of habit strength when amount of reinforcement is not optimal (M') is stated to be less than 100 habs. The writer feels that Hull is not justified in referring to M' as having less habit strength than M (say 80 habs) because it represents a lesser speed of

[8] The cross-section curve shown on this surface is not quite the same as that represented in Fig. 1 as in the region beyond 16 units incentive it is drawn from the data of Experiment III instead of being an average curve from three experiments. This procedure was followed because all the longitudinal curves are from Experiment III.

response. M' being the limit of response with unlimited practice at a particular amount of reinforcement is, like M, *complete* learning. But since the amount of incentive obtainable in this case is low, motivation is low, and consequently speed of response suffers. The learning or habit strengths may be for both M and M' substantially the same. The performances are different because the drives are different. The differences seem to be more, if one can be anthropomorphic for the moment, in *desire* to reach the goal, not in acquired skill necessary to reach the goal. Both groups have mastered the requisite skill.

VII. An Eagerness Theory of Incentive Motivation

Hull might retort to the arguments which have been given that *drive has been held constant* for the groups receiving different amounts of reinforcement. Consequently differences in level of performance cannot be attributed to this factor. But has drive been held constant? True hunger drive has been held constant, and very carefully. Is this, however, the only source of drive the organism possesses? The writer wishes to present the thesis that *emotional drive* has been varying among the groups of animals running to the different amounts of incentive, and this factor rather than simply learning is the major determinant of differences in levels of performance. With varying incentive amounts, after they have been experienced of course, there arise among the groups of animals varying amounts of anticipatory tension or excitement at the prospect of their acquisition. This is a differential of what may be termed *eagerness*. This differential of eagerness, of emotional tension or anticipatory excitement for the various amounts of incentive generates, according to this hypothesis, the differences within each trial in the obtained curves of performance. The larger the incentive amount for which an animal is performing the more eager he is to attain it, *i.e.*, the more emotional drive is summating with his hunger drive to motivate his response. Eagerness is related to learning only in the sense that the animal must find out how much incentive he is obtaining before he exhibits a corresponding amount of eagerness.

The eagerness value associated by an animal with a particular amount of incentive, *after he has come to expect it*,[9] is by hypothesis a function of the appetitive range of the animal. A particular amount of incentive is appraised as large, moderate, or small according to whether it results in a large, moderate, or small diminution of hunger pangs. This appraisal generates a corresponding amount of eagerness at the prospect of attainment.

The argument so far for an emotional drive or eagerness determinant of the differences of level of performance associated with the receipt of different amounts of incentive lies in its plausibility and the weakness of Hull's alternative (which will later be shown). There is, however, some qualitative data tending to support the thesis. In running rats to 16, 64, and 256 units of incentive the writer observed that the 64 and the 256 unit group were considerably more excited when being brought to the starting box, in the the food box—more eager—than was the 16 unit group. No appreciable difference was noted in the eagerness of the 64 compared to the 256 unit group. But then it must be remembered (*vide* Fig. 1) that their levels of performance were not very much different either. To crude observation any subtle differences in eagerness would be lost. In another experiment the one

[9] In a later section of this paper it will be indicated that eagerness bears a more complex relationship to incentive amount if the amount is greater or less than that expected.

and four unit incentive amount groups showed less eagerness for a time than the larger incentive amount groups. But this was complicated later, particularly for the unit incentive group, by rising excitement associated with minute incentive frustration.

Further support to this emotional drive interpretation of incentive motivation is obtained by a comparison of the effects upon behavior of food incentive variation with those of food drive variation. If, as the writer contends in disagreement with Hull, the apparently different situations are really both cases of drive variation, broadly similar results should obtain. Conveniently, such a comparison is possible as Hull discusses in detail the effect upon action of food drive variation (5, ch. 14).

Hull's discussion of drive variation is based upon data by Williams (15) and Perrin (9) relating number of hours of food privation (drive) to number of reinforcements and level of behavior. The measure of the latter was in this case the number of unreinforced responses which albino rats would make before their response was extinguished. Hull drew (5, p. 229) a three-dimensional graph or surface depicting behavior as a joint function of the other two variables, amount of food drive and the number of reinforcements of the habit in question. Now if incentive variation operates through the same mechanism as drive variation, as might follow from the writer's contention that both are really drive variations, the incentive surface of the writer's (Fig. 3) should be similar[10] to Hull's drive surface.

The comparison of the two surfaces—Fig. 3 of the present paper and Fig. 51 of Hull's text (5)—appears to reveal significant similarity. (1) Both graphs show positive growth curves between behavior of the animal and number of reinforcements. (2) Both show different growth curves for different amounts of the motivation variable (amount of food or amount of deprivation time). (3) For both surfaces the asymptotes of the longitudinal curves (*M*'s) are increasing functions of the motivation variable, but the rates of approach (*F*'s) are constant. The only major difference besides some zero-end extrapolations (which can probably be attributed to the difference in the indices of behavior employed for the dependent variable) is that, for the incentive surface, level of performance is not a slightly positively accelerated function of the motivation variable, as in Hull's (5, Fig. 49), but is negatively accelerated. This last difference does not weaken the case for the writer's suggestion that the effect of incentive variation upon performance is really through the mechanism of drive variation. This difference, using Hull's own reasoning, *strengthens* the argument. For Hull says in his theoretical discussion of the relationship of drive intensity to the conditions which produce it (5, p. 240), "It seems likely, on the basis of various analogies, that, other things equal, the intensity of the drive stimulus would be some form of *negatively accelerated* increasing function of the concentration of drive substance in the blood" (Italics the writer's).

Hull further cites the elaborate study of Warden and his associates (13) to the effect that hunger drive up to two or three days of food privation is a negatively accelerated function of time.

The comparison just completed and the preceding arguments make plausible the hypothesis that variations of amount of incentive operate to influence performance through the agency of varia-

[10] They need not be identical even if a common drive mechanism underlies the two as the behaviors in question are somewhat different, but to the extent that there is a similarity, the writer feels, his contention gains strength.

tion of emotional drive or anticipatory eagerness. If this is granted the differences in levels of action in Fig. 2 occasioned, over a series of trials, by different amounts of reinforcement cannot be looked upon as caused by differences in habit strength or learning alone, but must be understood as owing also to differences in drive strength. This point Hull understands very well in regard to Williams' and Perrin's data wherein drive is obviously varying. In these cases he terms the behavior not an index of learning alone ($_sH_R$) but an index of performance, or in his terminology reaction potential ($_sE_R$) which is determined by a function of drive [$F(D)$] as well as by a function of habit strength [$f(_sH_R)$]. The same conceptualization should be applied according to the writer's hypothesis to cases of incentive variation, for here too drive is varying though perhaps not so obviously.

The major theoretical discovery that Hull feels is indicated by his three-dimensional analysis of Perrin's data is that the relationship between drive, reaction-potential, and habit strength or learning is *multiplicative, i.e.*, $_sE_R = f(_sH_R) \times f(D)$. It adds no small weight to the writer's contention that incentive motivation should be included in the concept of D to find that with such inclusion apparently the same equation substantially holds.[11] This multiplicative relationship means that with any cross-section cut of Hull's surface—$f(_sH_R)$ is thus made a constant—variation of $_sE_R$ is occasioned entirely by $f(D)$. With a longitudinal cut—$f(D)$ is thus held constant—the variation in $_sE_R$ is occasioned entirely by $f(_sH_R)$. Further, this multiplicative relationship implies that these functions, $f(_sH_R)$ and $f(D)$, are the same wherever the cut is taken.

[11] It must be kept in mind, however, that the effect of amount of incentive upon the learning function does not become consistent until after a few opening trials. This is easily understood as the experience necessary for the different incentive amount groups to build up the different amount expectations and thus to get the different associated eagerness values operating consistently.

VIII. CONTRAST-SHIFTS IN AMOUNT OF REINFORCEMENT AND LEVEL OF PERFORMANCE

This final section of the paper is concerned with the effects upon levels of performance of shifting animals which have been accustomed to one amount of reinforcement to another amount, both when the second amount is greater and when it is less. Hull discusses what would follow under these circumstances from his 'learning' theory of incentive motivation by the use of which he has explained the findings discussed in this paper about the differences in levels of performance associated at the limits of practice with different amounts of reinforcement. This theory assumes that (1) the "successful reaction will be more strongly conditioned to the stimulus aggregate arising from a large piece of food than to that from a small one," and (2) "Therefore given a normal hunger drive, the organism will execute the correct one of several acts originally evoked by the situation more promptly, more vigorously, more certainly, and more persistently when a large amount of food is stimulating its receptors than when they are stimulated by a small amount" (5, pp. 131–132). The writer has already rejected this strength-of-conditioning theory of incentive motivation in favor of an emotional drive or eagerness theory on the grounds, it was stated, of the weakness of Hull's theory. It is with regard to shift of incentive results, it will presently be seen, that Hull's view seems inadequate.

Hull poses in his discussion the problem of an upward shift of incentive (5, pp. 129–130 and Fig. 29). If a group of animals are given one gram of rein-

forcement over a number of trials their level of performance will describe a learning curve or growth function which reaches an asymptote—the one-gram limit of performance. Similarly for a six-gram group except that the six-gram limit will be higher in habit strength (greater speed in a runway for example). Now Hull says, if, after 15 trials, the animals receiving one gram of food are shifted to six grams the change in their performance will describe a growth function whose asymptote is the *six gram* limit.[12]

In his chapter on amount of reinforcement Hull does not give detailed consideration to the parallel case where amount of incentive is suddenly reduced. He states instead that it must be deferred until experimental extinction has been discussed. But, unfortunately, as far as the writer can ascertain, the topic has not been treated. The implication of Hull's theory seems to be, however, that with downward shift of amount of reinforcement, say from six grams to one gram, the level of performance would descend to the one gram limit.

What happens to these implications of Hull's learning theory of incentive motivation? If they are not borne out, the theory must be brought into question. In the light of experiments of the writer on precisely this problem, effect of shift of incentive amounts upon level of performance, Hull's deductions are sharply controverted. For groups of animals shifted upward from both one unit and four unit incentive to 16, and downward from 256 unit or 64 unit incentive to 16, the resulting changes in level of performance are definitely contrary to those demanded by Hull's theory. These facts can be clearly seen in Figs. 7 and 8 of the writer's paper (2, p. 508). Both of the groups which were shifted upward in amount of reinforcement substantially and reliably *shoot above* the level of performance characteristic of the 16 unit group at the limit of practice. This is what the writer has termed the 'elation effect.' Similarly both groups shifted downward in amount of reinforcement substantially and reliably *drop below* the level of performance characteristic of the control group at their limit of practice. Here there is what the writer has termed the 'depression effect.' These elation and depression effects run definitely counter to the results anticipated by Hull's learning-theory of incentive motivation, but the writer feels they fit in well with his own emotional drive or eagerness theory.

According to the writer's proposal as has been previously stated, a particular incentive amount, *after an animal has come to expect it,* bears a particular eagerness value or anticipatory excitement. What this value is depends upon the relation of the amount of incentive to the appetitive range of the animal concerned. This amount of eagerness is the emotional drive associated with a particular amount of incentive when level of achievement is *equal* to level of expectation. But when level of achievement is appreciably *greater,* for example, than level of expectation, as is the case with upward shift in amount of reinforcement, background emotional drive gets a boost aroused by the discrepancy (elation or joy), and the vigor of performance is increased.

One has only to reflect upon his own reactions to see how reasonable an assumption is this elation boost in emotional drive. If one is doing a task for a dollar reward and suddenly and unexpectedly receives five dollars for the same task, he is likely to be elated and his level of performance will rise. There is no reason to expect other animals to react dissimilarly.

[12] This contention is clearly evident in the equation that has been given for the shifted group's growth function (5, p. 134).

A boost in emotional drive because of an unexpected increase in reward may account for the elation effect in level of performance, the depression effect with one additional complexity may be accounted for similarly. When an animal suddenly receives a reward less than he expected for a certain task, there is also, the writer believes, a *boost* in emotional excitement, but what might be anthropomorphically termed anger instead of joy. The emotional surge of stimulation under these circumstances does not pour into the activity at hand to increase its vigor as in the elation effect, but pours into various goal-avoidant activities or frustration symptoms. This is not just speculation but is suggested by the following considerations from the writer's experiments. In the 16 unit incentive control group when performance had leveled out most of the rats went directly down to the food box. Occasionally there was pausing here and there in the runway, but there were very few long steps or retraces. Peering had by this time become rare. The first experience of change by the rats shifted down to 16 from 64 units incentive brought general excited peering in the food box, general delayed eating, one refusal to eat, and one persistent attempt to jump out of the food box. In subsequent trials, considerable peering and retracing around the starting box quickly arose, as did also retracing and jerky, hesitant locomotion. Among some rats prolonged smelling of the pathway and biting of the starting door were in evidence. The first experience of change among the rats shifted downward to 16 from 256 units of incentive occasioned general frantic peering, general delayed eating, repeated jumping attempts to escape the food box in three cases, and refusal to eat all or part of the incentive in three cases. Upon subsequent trials there was prolonged peering at the starting door, jerky hesitant locomotion, retracing, prolonged smelling, and door biting. On the first experience of the incentive shift, at least, the 256 unit incentive group exhibited substantially more disruption of behavior than did the 64 unit group (2, p. 510).

To summarize, in both the elation and depression effects activity appears to be augmented by a heightened emotional drive. But in the one case, elation, the added stimulation summates algebraically to increase the vigor of the goal activity above the level that would be the consequent of ordinary eagerness associated with the shifted-to amount of incentive. In the other case, anger, the added stimulation summates algebraically to decrease the goal-directed vigor below the level that would be the consequent of ordinary eagerness associated with the shifted-to incentive. The diminution of activity, discussed previously in this paper, associated with the receipt of minute rewards is also a case in which emotional drive, anger, aroused for reasons that have been given, increases general activity but decreases goal-directed activity.

An implication of this hypothesis which invites verification is that the elation and depression effects after a number of trials should trail down to the level of performance characteristic of the shifted-to amount of incentive. This follows since with experience level of expectation would be brought into line with the new level of achievement, and consequently the emotional boost in drive due to their disparity would disappear, leaving as incentive motivation only the eagerness value characteristic of the particular amount of incentive in terms of the appetitive range of the animal. Examination of Fig. 8 (2, p. 508) of the writer's previous paper suggests that this implication will hold, since around the eighth post-shift trial the depression effect is already trailing back toward the level of performance char-

acteristic of the shifted-to amount of incentive.

It is fitting to append to this appraisal of Hull's treatment of the relationship between amount of incentive and behavior a few remarks in regard to the spirit in which the criticisms are made. As Hull has stated, "A sharp and definite formulation has, in many cases, been given principles despite admitted doubt as to their precise validity. It is believed that a clear formulation, even if later found incorrect, will ultimately lead more quickly and easily to a correct formulation than will a pussyfooting statement which might be more difficult to convict of falsity" (5, p. 398). With this point of view the writer is in agreement. It is not a little due to Hull's precise formulations that the discrepancies between his treatment and that of the writer became apparent, and thus the need pointed out for their resolution. If Hull's discussion in the light of the criticisms of this paper can be looked upon in some respects as no more than a first approximation, the writer's is certainly no more than a second.

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SOCIAL PERCEPTION AND PHENOMENAL CAUSALITY

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I. Introduction

In recent years a great many studies have been made of the processes of organization in the perceptual field. It is the thesis of this paper that the principles involved in these studies can be applied profitably to the perception of other persons and their behavior, and that one of the features of the organization of the social field is the attribution of a change to a perceptual unit. When we see a moving object A, we can attribute the movement either to A itself or to another object B. In the first case we see the movement as a spontaneous act of A, in the second as passive movement induced by B. When we have a disagreeable experience, or a pleasant one, we may locate its origin in another person, in ourselves, or in fate. An attempt to investigate the role of these attributions is described in a study of 'apparent movement' in which a motion picture of geometrical figures was presented to groups of subjects (16). This study showed how causal attributions influence the perception of the behavior of other persons. The relatively abstract nature of the material used in this experiment made it possible to study some of the more elementary factors of causal attribution in social perception; in the present paper an attempt is made to analyze these processes further and to show their influence in a variety of more concrete cases of social behavior. The discussion will not be restricted to purely perceptual processes but will include processes like 'apperception' or 'cognition.' Since the organization of the social environment seems to follow similar laws whether or not the data are given perceptually it seems, in this case, profitable to treat perception and apperception from a common point of view.

Making a first approach to this analysis we may say that the origin and the change which is attributed to the origin form a unit; that is to say, the change 'belongs' to the origin. The connection between origin and change is in many cases manifest and appears phenomenally as causal dependence: "The origin causes the change." Therefore, origin attribution is a case of 'phenomenal causality' (11, pp. 78 ff.), or 'causal integration' (46, pp. 679 ff.).

Ryan, in discussing the case in which an object is perceived as the source of a sound, says as follows:

> Such an attribute of 'being a source of sound' is certainly neither a pattern of qualities (either visual or auditory) nor a spatial attribute. It is a property which belongs among what we shall call 'dynamic' characteristics of an object. Such properties can be as directly perceived as color and play a great part in integrating the total sensory mechanism (46, p. 679).

The difference between what Ryan here probably means by the term 'pattern of qualities' and causal integration can be demonstrated in the field of vision. When we see a color as belonging to an object, the color is perceived as a property of the object, and this object unit with its properties could be called a 'pattern of qualities.' But when we see a color patch as a spot of sunlight, the color patch is not a property of the sun. In both cases we find an integration, although this integration is of different degree. The pattern of qualities which corresponds to an object unit is more closely knit than the unit produced by causal integration in which

origin and effect are relatively independent parts.

Such causal effects often play the role of data and can be thought of as proximal stimuli through which are mediated to us properties of the origins which belong to the stable relevant psychological environment. Through causal integration changes or passing experiences are related to the framework of the invariant environment which gives the change its meaning. Thus, origin attribution is a case of the organization of experiences and has its roots in the 'individual's pursuit of meaning' which has been discussed as a problem of social cognition in relation to standards of judgments and social values (8, pp. 53 ff.).[1]

II. PERSONS AS ORIGINS

That animate beings, especially persons, are the prototype of origins has been amply demonstrated by many writers.

> In earlier and simpler ages men knew little and frankly referred the explanation of what they could not account for on the basis of concrete experience, to the world of spirits which, as they conceived of it, surrounded and permeated their existence. In so doing they regularly sought for an agent rather than a cause (54, p. 654).

In the development of the child's causal thought "the original union of doer and deed forms the schema according to which causal thought can develop" (51, p. 409; this sentence has been omitted from the English translation). The child has the conviction that "all changes that concern me (actively or passively) are 'made to happen'" (51, p. 410).

The tendency to attribute changes to personal origins is, of course, related to what Piaget calls animism, that is, the tendency to attribute life to inanimate objects.

Animism, or personification, has been explained in different ways. The most common explanation refers to an inference based on analogy and is similar to those theories which contend that we understand other persons' minds because we conceive them as analogous to our own mind. Similarly, it is said that in personification the child attributes to things properties which it has experienced as belonging to his own person, or to persons of his environment.[2]

Werner rejects this theory. He believes that "personification in a primitive interpretation of the world is derived from the physiognomic mode of expression and perception" (61, p. 75). By the term 'physiognomic perception' he understands a preference for interpretation in terms of dynamic rather than static properties.

> It may be that the child apprehends persons physiognomically more readily than other objects in his surrounding world. This fact might give rise to the erroneous impression that the child first discovers physiognomic characteristics in human individuals and then transfers them to nonhuman objects. The more direct assumption, however, and one which is in greater accordance with the facts, is that the child, grasping the world as he does through his motor-affective activity, will understand the world in terms of physiognomics before personifying (61, pp. 75 f.).

Personification arises out of this physiognomic perception when "the child's need to handle objects in his everyday life as if they were persons comes into being. For instance, he wants something from the inanimate ob-

[1] Some authors talk of a general tendency towards causal explanation, a 'causal drive.' Oppenheimer (42, pp. 238 ff.) considers it as a third basic drive beside the drives for self conservation and for the conservation of the species.

[2] Dennis (9) offers quotations from authors who held this view. For arguments against this kind of theory in regard to the understanding of other persons, cf. Köhler (28, pp. 233 ff.) and Koffka (27, pp. 655 ff.).

ject, and as a result of this desire it inevitably comes to personal life. The child can become angry with it, or sympathize with it, or punish it. Personification is engendered principally by a specific attitude which the child must adopt in dealing with the objects of his world in a 'social' fashion" (61, pp. 77 f.).

In this explanation it is a need, for instance, a need for revenge, which requires an object and thus leads to personification. That this is not always the case is shown by experiments on apparent behavior. Personification can be compelling in the absence of such needs.

Another explanation besides those in terms of experience or needs seems to be possible. The prevalence of personification in an imperfectly structured environment might be caused, at least in part, by the simplicity of origin-organization. The changes are attributed to a single concrete unit as source, which is certainly a simpler organization than that to which an analysis in terms of a causal nexus with many conditions would lead to.[3]

Discussions of the concept of responsibility would tend to support this point of view. We find in them repeatedly stressed the difference between a more differentiated causal analysis and personal causation in which an effect is attributed in an absolute way to one origin, the causal chain terminating in it. Fauconnet, in presenting the views of the classical school of indeterminism, says:

> There exists a causality peculiar to man, different from the causality which connects natural phenomena. Man is, in a certain sense, a first cause, if not of the physical movements which constitute his acts, at least of their moral quality. . . . From this perfect causality originates his responsibility. . . . Be it good or bad, the act is mine exclusively and absolutely (12, pp. 177 f.).
>
> A true antagonism exists between this idea [the idea of human causality] and the scientific notion of causality. First of all, science knows only of secondary causes: every cause, in itself, is also an effect, and causal explanation is a regression which has no conceivable end; secondly, the cause is for science a phenomenon like the effect, the laws of causality only describe necessary relations between phenomena. On the other hand, the person conceived as cause is a first cause; the act, said Aristotle, has its beginning in itself; secondly, the two terms are here heterogeneous, between them exists, not the relation of phenomenon to phenomenon which a law expresses, but the relation of producer to product, of workman to finished work (12, pp. 277 f.).[4]

If we conceive of the idea of 'person' in this way, light is also thrown on the theory that personification originates in needs. Interesting in this connection is Fauconnet's description of the development of the concept of responsibility. He says, following to some degree Durkheim whose pupil he is, that crime violates society and menaces its life. It must be annihilated so that the moral order can be rehabilitated. But the crime is a *fait accompli;* it cannot be annulled. Therefore, a substitute has to be found, a symbol of the crime whose destruction replaces the destruction of the crime, and the beings which are taken as substitutes are considered responsible for the crime.

[3] *Cf.* J. M. Baldwin's description of the development of a child's idea of person (2, p. 124).

[4] *Cf.* Stok (52, p. 357): "Things appear as homogeneous entities. . . . The person, on the other hand, is a final, heterogeneous causal center (*Wirkzentrum*); everything that occurs in its region is thought of as coming from it, and only from it. All lines which one follows backwards intersect in one point, no line points beyond. The world of relations ends with the other person. The field of explanations is, in regard to personal events, convergent." Also MacIver (34, pp. 225 ff.).

> Is a first and personal cause anything else but a cause conceived in such a way that it can be held responsible, that it can furnish something fixed and constant to which sanction can be applied? (12, p. 278).

These theories, whether or not entirely correct, seem to illuminate the meaning of the unit 'person' in our life space. Through these units the organism is enabled to reinstate an equilibrium even when otherwise irreversible changes have disturbed it. Persons, as absolute causal origins, transform irreversible changes into reversible ones. We understand now why the need for revenge, a special case of a need for the reinstatement of an equilibrium, requires a person as object: because of the simplicity of organization, the person can represent the disturbing change in its entirety.

That this tendency to perceive persons as origins influences social perception can be shown by many examples. It can lead, for instance, to an underestimation of other factors responsible for the effect. Changes in the environment are almost always caused by acts of persons in combination with other factors. The tendency exists to ascribe the changes entirely to persons. This is shown by the reaction to other persons' successes or failures. The interpretation will, of course, depend also on forces acting on the 'alter level,' that is to say, on the value level of the other person within our life space. If we are inclined to disparage him we shall attribute his failures to his own person, his successes to his good luck or unfair practices. But disregarding these forces, there probably exists a tendency to be 'intropunitive' in regard to other persons, that is, to see the cause of their successes and failures in their personal characteristics and not in other conditions.

When Nietzsche says, "Success is the greatest liar," he refers to this error in attribution. Klages, in his book on Nietzsche, devotes a whole chapter to the suasion of success, in which he treats the fact that success convinces us of the worth of a person even if the success is largely due to chance circumstances (26, pp. 85 ff.). Ichheiser makes the same point in several of his papers (21, 22).

Stern's concept of 'personal causality' also refers to this exaggeration of the influence of personal causal factors:

> "By this term we understand [with Stern] the causal connection between the doing subject and his deed. When A performs an act by which the death of B is caused, we say: 'A has killed B.' We say that, even if there are intermediate links between the act of A and the succeeding death of B . . . which are outside the sphere of A (environmental factors). It still is the act of A which led to the death of B. This conception is certainly much more primary than the analysis of an action into intention of the actor, environmental factors, and final effect" (56, p. 97).[5] In the same way the layman is apt to exaggerate the importance of the personal factor in the interpretation of historical events though with some historians we can also find one-sided attribution to impersonal forces (*Cf.* 20).

The same tendency can be seen at work in our perception of the conditions which are responsible for an act. Often the momentary situation which, at least in part, determines the behavior of a person is disregarded and the behavior is taken as a manifestation of personal characteristics. Argelander discusses the different ways in which this fact can lead to wrong judgments of personality.

[5] *Cf.* also 63, Ch. IX: 'The will as the subject of moral judgment and the influence of external events.'

The trait of character which will be ascribed to a person depends on the situation in which the person's behavior is being observed (1, p. 285).

In *She Stoops To Conquer* Goldsmith presents a young man whose behavior varies extremely with the momentary situation. The other characters attribute the temporary behavior to the permanent personality and this error produces many of the comic situations of this play.

III. The Relations Between Causal Units and the Properties of Their Parts

In the following sections we shall discuss the role causal attribution plays in the cognitive structure of the environment. We have to consider the factors which affect attribution, and the influence which causal integration exerts on the appearance of the parts. Following a proved procedure (*Cf.* Koffka, 27; Lewin, 33), we shall first consider the forces resulting from the structure of the cognitive field itself, and secondly the forces which relate to motivations and needs.

A. The relation between the parts as a factor in causal unit formation

Origin and effect, or a person and his act, are the parts of a causal unit. The properties of the act may be similar or in contrast to those of the person. This is possible since we often judge both persons and acts in reference to the same dimension. Many trait names which we use in describing persons can also be used in describing acts. Persons or acts may be altruistic, vicious, or brutal. This is especially true for value properties: a person or an act may be good morally. Applied to a person the adjective refers to a dispositional concept, for instance, to a permanent faculty; applied to an act it refers to a temporary event.

Therefore, the relation between an act and a person can be one of varying degrees of similarity. We know that similarity is a factor in figural unit formation (62). The same holds true for causal unit formation. In general one may say that factors making for figural unit formation also make for causal integration. Duncker (11) and Huang (20) have pointed out the importance of similarity and proximity in phenomenal causality. If two events are similar to each other, or near each other, one is likely to be perceived as being the cause of the other. The literature on magic practices is also relevant here since magic actions are based on primitive causal ideas about the environment. Tylor (57) and Frazer (13) have shown what role similarity and proximity play in this respect (*Cf.* also Köhler, 29).

That similarity is a factor in social causal integration is demonstrated by Zillig's experiments (64). She showed that poor performances will be attributed to unpopular children by their classmates. A bad act is easily connected with a bad person.

Fauconnet says that all situations which give rise to the idea of responsibility (the attribution of a crime to a person) can be classed in two groups: responsibility by contiguity and responsibility by similarity (12, p. 261). The latter includes several varieties. A crime can be blamed on a person because of physical similarity "he looks as if he could have committed this crime."[6] Or he can be held responsible

[6] *Cf.* the Toscanini anecdote related by Oscar Levant in *Harper's*, 1939, 179, p. 589: "One such unfortunate, a violinist, was the invariable target of his criticism, because of his mottled complexion. During a visit by the orchestra to Hartford, someone in the violin section made a false entrance, and Toscanini, in a rage, placed the blame squarely on his *bête noir*—even though the musician had remained in New York, ill." (*Cf.* also 4, pp. 165 f.)

for it because of 'spiritual' similarity, that is, a similarity between a 'crime as a moral event and the natural disposition of the responsible.' "Can one discover in the accused a basic immorality comparable to that of which the act is held to be a symptom, does one find his crime in him, his character in his crime, in short, does he resemble his crime? His responsibility is not complete unless the answer is affirmative" (12, p. 264).

The responsibility can also be established by 'affective similarity.'

> It is easy to assume that persons are responsible who are not loved. In a general way, 'antipathy' arouses suspicion; and a special 'antipathy' arouses a special suspicion. Persons dreaded for their brutality are the first ones to be suspected of a violent crime; despised persons, of a mean act; and those who arouse disgust, of an unclean act. People with bad reputations are accused and convicted on the basis of evidence which one would consider insufficient if an unfavorable prejudice did not relate them to the crime in advance. On the contrary, if the accused has won our favor we demand irrefutable proof before we impute to him the crime (12, p. 266).

The property which makes a person in our eyes 'similar' to a crime or an act can be created by past experience: we know that he has committed such an act before. In causal attribution the factor of past experience cannot be separated from the factor of similarity because a person, once having been the origin of an act, takes over its quality. The person is then connected with the act also by the relation of similarity. The difficult rehabilitation of people who once were inmates of a prison or a mental hospital, or the fable of the boy who cries 'Wolf' are relevant examples.

That a crime can be judged as 'belonging to' a person on the basis of irrelevant circumstances is shown by incidences related by Gross:

> So for example, an intelligent man told me that he suspected another of a murder because the latter's mother died a violent death. The witness stuck to his statement: "the man who had once had something to do with killing must have had something to do with this killing." In a similar manner, a whole village accused a man of arson because he was born on the night on which a neighbouring village burned down. Here, however, there was no additional argument in the belief that his mother had absorbed the influence of the fire inasmuch as the latter was told that there had been a fire only after the child was born. "He once had something to do with fire," was the basis of the judgment, also in this case (15, p. 122).

In these cases the person takes over, by assimilation, a 'whole-quality' of a crime with which he was in some way connected. Then, the factor of similarity causes a new crime of the same sort to be attributed to him. Thus the factor of proximity can make for an assimilation of a person to an event. It is not so easy to find a clear case in which this factor is directly the cause of an unjustified attribution of an act to a person. The cases which Fauconnet cites as examples of 'responsibility by contiguity' cannot always be called errors of attribution,—for instance, the fact that the presence at the place of crime speaks against the accused in the eyes of the jury; . . . nor are they always clear cases of attribution,—for instance, the fact that a person is held to be contaminated by a contact with the criminal (pp. 261, 262).

B. The influence of causal integration on the relations between parts

1. *Assimilation and contrast.*—It is known, from the study of elementary perception, that the parts of an integrated structure will tend either to look as much alike as possible, or to look as much unlike each other as possible. Werner, who treats these phenomena in regard to intensity, says:

This organization occurs in the most important cases either in such a way that a part or whole property of the structure influences the intensity directly by assimilation, or so that it affects the intensity by a relation of opposition (*beziehendes Gegenüberstellen*). In the first case . . . we speak of a dependence of mixture or assimilation, in the second of a dependence of segregation or dissimilation (59, pp. 47 f.).

Gestalt psychologists talk in this connection of the maximum-minimum principle, that is, the tendency towards maximum or minimum simplicity. ". . . we must remember that there exist always two possibilities, corresponding to the minimum and the maximum; either as little or as much as possible will happen" (27, p. 109). "Roughly speaking, a minimum simplicity will be the simplicity of uniformity, a maximum simplicity that of perfect articulation" (27, p. 171). Werner (59) and Koffka (27, pp. 171 ff.) see in assimilation and contrast two different expressions of the tendency towards *Prägnanz*. Examples are given by Werner (60).[7]

2. *The influence of the origin on the effect.*—In social perception, the act is in many cases assimilated to the origin. Acts or products are colored by the qualities of the person to whom they are ascribed. A joke made by a person considered silly will usually seem silly, while the same joke made by a person with the reputation of being witty will arouse laughter.

When the worship of Mammon is widespread the millionaire is a high authority, not only in the technique of money-making, but in everything else as well. . . . His crassest stupidities have the indescribable charm of coming from a 'solid' man (44, pp. 175 f.).

The phenomenon of 'prestige suggestion' is familiar to social psychologists: the acceptance of statements and the evaluation of artistic products depends on the prestige of the person to whom they are attributed (47, 49).

Contrast or dissimilation occurs when the origin is taken as the standard for judging the quality of the act. We can assume that in these cases the fusion between act and origin is less complete and the act is perceived, so to speak, with the origin as background. Our reaction to praise or any personal comment depends greatly upon whether it comes from a reserved or a loquacious person. Shakespeare makes use of this kind of contrast when he describes Othello as a person to whom jealousy is foreign. If he had introduced Othello as a man inclined to be jealous, his acts of jealousy would have lost much of their dramatic force (Cf. 53). Sophocles' Philoktetes offers a similar example. Philoktetes is presented as possessing a high degree of self-control. That such a man cries out loud in pain is very impressive.

It has been remarked that visual contrast phenomena are related to color transformation (usually called, in a misleading way, 'color constancy').[8]

A proximal stimulus of medium intensity seen in dim illumination signifies a very white object. This relation is similar to that in the examples taken from social perception. A reaction of medium intensity coming from a person

[7] For an experimental demonstration of assimilation see also Fuchs (14) and Campbell (7). For conditions which determine whether assimilation or contrast will be effective, *cf.* Koffka (27, p. 173, explanation in terms of disposable energy of organism), and Werner (59, p. 115; 60, p. 252; assimilation occurs with high degrees, contrast with lower degrees of integration). Usnadze's findings with experiments on sets corroborate Werner's opinion: assimilation appears when the difference between members is small, contrast when it is great (58, p. 332).

[8] *Cf.* Jaensch's thesis: Laws of contrast become laws of transformation if the term 'surrounding field' is replaced by the term 'illumined space' (24, p. 272). Also Werner (59, pp. 120 ff.).

whose usual reactions are known to be feeble, will signify a strong environmental 'press,' to use Murray's term. This relation is similar to that in the examples just mentioned of Sophocles' portrayal of Philoktetes' pain.

3. *Influences of effects on origins.*—Changes will influence the appearance of an origin if they are combined with it in a causal unit. Effects often play the role of data through which we learn to know about the origins, and it is obvious that our idea of a person can be influenced by the attribution of acts. But here, again, we have to note a difference between a causal unit and a simple object unit. We saw above that in the latter a color is directly ascribed to the object as a property. In the less highly integrated causal unit the change does not appear as a property of the origin, only the possibility or faculty of producing this change is ascribed to the origin as a property.

At the same time, the origin is assimilated to the produced effect. Effects can be the determining elements in the whole-qualities of origins which are permeated with the color of the effects. As examples may be mentioned poisons, 'hot' pepper, alcohol, or explosives. In the same way a person who is seen as the origin of a bad act is usually judged to be bad himself. MacIver mentions as one stage in the imputation of responsibility "the imputation to the conscious agent of an attitude or disposition, perhaps we might say an *animus,* which makes not the doing only but the doer susceptible to a value judgment" (34, p. 226).[9]

[9] The influence of causal attribution on our attitudes is also referred to in Spinoza's system: "If we remove a disturbance of the spirit, or emotion, from the thought of an external cause, and unite it to the other thoughts, then will the love or hatred toward that external cause, and also the vacillations of spirit which arise from these emotions, be destroyed" (50, p. 254).

Contrast does not seem to occur as influence of act on person. The unit of origin and effect is a hierarchical unit: the origin is a superordinate part, the effect a subordinate one. Therefore, the effect can be seen with the origin as background and the latter can provide a standard according to which the meaning of the effect appears, but the reciprocal relation (origin with effect as background) is impossible.

However, the effect can be evaluated in reference to an environmental 'press,' and thereby something like a contrast phenomenon can arise. If we perceive that a person stays moderately calm under extremely exciting circumstances we shall judge him as very quiet. Just as our perceptual mechanism 'knows' that the light reflected from a surface is dependent on two conditions, and represents it as a combined effect of illumination and object color, so we 'know,' in many cases, that the behavior of a person is dependent on situation and personal characteristics. Sometimes behavior and personal characteristics are given and the press value of the situation is inferred (on the perceptual, not rational level), as in the above mentioned example of Philoktetes; sometimes behavior and situation are given and we infer personal characteristics. In these cases we find dissimilation between personal origins and effects.

Often, the change that our idea of a person undergoes when we attribute an act to him cannot be described so simply in terms of assimilation or contrast. When we see a person lift a heavy weight we perceive at the same time that he is strong. 'Lifting a heavy weight' and 'being strong' belong together as manifestation and faculty but they can hardly be called similar. These faculty concepts play an important role in social apperception. For instance, we often have a definite idea of what a person—our own person or

another person—is able to do, or not to do.[10] This 'being able to do something' is ascribed in part to the environment; for instance, the possibility of moving around freely in a region is ascribed to the fact that the region is without barriers. But it is also ascribed in part to the person, especially, when achievement or failure to do something deviates greatly from that of average people.

Ichheiser says: "It may be said with assurance that the knowledge about kind and degree of the personal basis of our own capability usually constitutes itself by way of real acts. Only in acting and in attaining goals, achievements, and successes do we learn to know what we can do" (22, p. 372).

Thus the change or act, something temporary, is correlated to something relatively stable and invariant, a possibility or capability. This is true in all cases of attribution to persons. Dynamic characteristics, too, can be ascribed in this way to persons as stable properties. If an act is seen as directed against us, we shall ascribe to the person whom we see as origin of this act a hostile intention which may be of a much longer duration than the temporary act. On the other hand, if we are convinced that the act does not originate in constant characteristics of the person we shall not hold him responsible.[11]

Essentially, these cases are not so very different from simple assimilation. The origin is changed by the effect in such a way that it 'fits' the effect, or, if one wants to use an analogy taken from figural units, in such a way that the effect is a good continuation of the origin. We can express this dependence in more general terms: If we are, for a certain reason X, convinced that A is responsible for act a, then we see A in such a way that A and a would be perceived as belonging together even if reason X were absent.

IV. Causal Units and Dynamic Factors

A. *The influence of attribution on the dynamic state of the person*

Emotions and tensions in the person are closely linked to facts of attribution. As soon as one attempts to determine the cognitive structure of the environment which is coordinated to emotional reaction one faces the fact of causal attribution. Spinoza's treatise 'On the origin and nature of emotions' in his *Ethics* is the most thoroughly worked out system in this regard. A great number of correct observations are treated in a theoretically unified way and attribution plays a prominent role in this system.[12]

This is understandable since many of these dynamic states do not refer to proximal stimuli *qua* proximal stimuli, that is to say, not to the effects as they reach our person but to effects referred to the framework of the permament psychological environment.[13]

[10] *Cf.* Lewin's concept of 'space of free movement' (31, pp. 42 ff.), or Ichheiser's 'characters of attainability' (*Erreichbarkeitscharaktere*, 22, p. 376).

[11] The interesting question to what parts of a person acts are attributed, and the errors of attribution in this respect, will not be treated here. *Cf.* Ichheiser (23) who, for instance, discusses the fact that we tend to overrate the influence of the 'rational' parts of our own person.

[12] *Cf.* also the classifications of emotions by psychologists who were influenced by Spinoza (Mercier, 38; Höffding, 17; Calkins, 5. Mercier's classification can be found in Ruckmick, 45).

[13] "It is necessary to be remarked, respecting the causes of our pleasurable and painful sensations, that they are apt to become greater objects of concern to us, to rank higher in importance, than the sensations themselves . . . the mind is not much interested in attending to the sensation. . . . The mind, however, is deeply interested in attending to the

In treating these phenomena we have to consider first the influence of attribution on the dynamic state of the person; in the next section we shall discuss the influence of the state of the person on attribution.

One can say that attribution influences the needs or emotions because the latter are correlated to the meanings of effects, and the meanings of effects are determined by attribution to an origin.

That the meaning of a datum is determined by the reference to an origin can be shown by many examples. Our reaction to a disagreeable experience, for instance of pain, is greatly influenced by the attribution to a source, which we may see in another person, in the workings of chance, or in ourselves. The same datum may mean aggression, misfortune, or a stupid mistake. When an injury is attributed to a personal agent, it is more likely to lead to an aggressive reaction. If we were convinced that the pain we feel at the dentist's had its ultimate source in the malevolence of a person instead of in the poor health of our teeth, we would react to it quite differently.

The same is true of the meaning of an experience of frustration. Usually, frustration leads to aggression only (and not always even then) when the origin of the frustration is attributed not to one's own person, or to impersonal causes, but to another person. Several of the critics of the book *Frustration and aggression* by the Yale group (10) point out the importance of attribution in this respect.

It is a question, however, as to whether numerous situations in which the individual does not deal directly with frustration readily translatable into terms of an aggressor, typically stimulate the aggressive rejoinder, for example, frustrations arising out of one's own inability to solve a mathematical problem and the like (30, pp. 357 f.).

Maslow has something similar in mind when he makes the important distinction between deprivation and threat to personality:

> Thus a certain child deprived of an ice-cream cone which he wanted may have lost simply an ice-cream cone. A second child, however, deprived of an ice-cream cone, may have lost not only a sensory gratification, but may also feel deprived of the love of his mother because she refused to buy it for him. . . . It is only when a goal object represents love, prestige, respect, or achievement that being deprived of it will have the bad effects ordinarily attributed to frustration in general (36, pp. 364 f.).

One could add that a frustration is experienced as a threat to the personality only when it is interpreted in terms of the social environment, that is to say, when persons are perceived as the origins of the frustration, and the temporary 'deprivation' is related to durable environmental relevancies.[14]

The influence of causal integration on the experience of success or failure offers another example of the determination of the meaning of a datum by origin attribution.

Not merely success or failure as such is dynamically of importance but above all the experience of belongingness of these effects to the own person, the fact that the

cause; that we may prevent, or remove it, if the sensation is painful; provide, or detain it, if the sensation is pleasurable. This creates a habit of passing rapidly from the sensation, to fix our attention upon its cause" (39, p. 188).

[14] That origin attribution has something to do with the reaction to frustration is conceded also by the Yale group: ". . . the strongest instigation, aroused by a frustration, is to acts of aggression directed against the agent perceived to be the source of the frustration . . ." (10, p. 39). *Cf.* also Koffka's brief description of experiments conducted by Dembo (27, p. 673) in which she compared anger directed against a personal cause and 'impersonal' anger. The first one was much stronger and its after-effects lasted longer.

achievement is considered as an expression of the value of the own person (19, p. 33).

In general it may be said that success or failure is experienced only when the source of the achievement (or, in the case of failure, the source of the inability to achieve a goal) is located in the own person. One of the devices used to lift morale is to restructure the field in such a way that a defeat is not attributed to one's own inferiority. Defeat undermines the morale of a nation or a person only if the cause of the defeat is attributed to its own weakness. When, on the other hand, the blame is laid on a 'stab in the back' or some other factor which is not connected with the relationship between the own power and that of the enemy, the self-evaluation which is basic for morale is not affected.

These examples show that causal attribution is of great importance in cognitive structures which give rise to tensions in the person since many vital equilibria concern relationships of the own person to other persons; relationships of power, of value, of benevolence or hostility. Most of the 'sensory' experiences of changes having a positive or negative value for the person become relevant to these equilibria only when they are, by attribution, related to the stable social environment consisting of other persons.

But this is not entirely true of all sensory experiences, just as it would not be entirely true in the field of object perception. There exists a 'stimulus directed' perception and the phenomenal representations of local proximal stimuli, in some cases, retain their identity in spite of their being organized into different object units. This is also true in regard to the experience of ego-relevant facts. Pain is disagreeable in itself and, though its coloration can be widely changed by different attribution, its core of negative value persists. The meaning of frustration is changed by causal integration to a greater degree than that of pain. However, even the effect of frustration is to some degree independent of how it is embedded in the social environment. Frustration caused by a person typically leads to anger; but also one not caused by a person can lead to anger (27, p. 673). Therefore, though not being universally correct, the 'constancy hypothesis' which lies hidden in the original theory of the Yale group has some limited justification.

B. The influence of attitudes on attribution

We have seen that the similarity between origin and effect, which makes for, or is brought about by, attribution, often refers to value properties which in turn are closely linked with dynamic attitudes. There exist tendencies to perceive ourselves or other persons in a certain way, and these tendencies affect origin attribution.

The tendency to keep the ego level high is an example. Hoppe showed how this tendency can influence attribution. His subjects frequently tried to clear themselves of the responsibility for failures. "Usually the blame is then put on the material. . . . One evades, as far as possible, the effect of the failure by looking for the cause of it not in the own person but in the neutral sphere of objects" (19, p. 33).

It is obvious that this tendency to keep the ego level high must play a role in attribution. Since origins are assimilated to acts attributed to them, an act of low value, when attributed to the ego, will lower the ego level, and an act of high value will raise it. However, this will happen only when the stimulus conditions are so strong as to enforce the attribution, that is, if there can be no doubt that the own person is the source. Often, the possibility of

different organizations will exist. Then the tendency to raise the ego level will structure the causal units in such a way that only good acts and not bad ones are attributed to the own person.

Examples demonstrating these effects are sometimes treated as cases of rationalization,[15] or projection. Claparède talks of excuse (denying the attribution of a bad act to oneself) and imputation (ascribing bad acts to others).

> When we have spilled ink on a lady's dress, or when we have committed a more grave sin, we consider calmly that it was not our fault, since the course of the world is determined inevitably, and this accident was already virtually contained in the structure of the primordial nebula.
>
> Imputation appears frequently in mental pathology in the form of imaginary accusation, etc. These facts are well known, and psychoanalysis has interpreted them as a means to end an interior conflict painful to the subject, by the projection on another person of sentiments condemned by conscience (6, pp. 281, 282).

Scapegoat behavior often is not simply release of aggression but includes blaming others for changes which, if attributed to the person, would lower the self esteem. Cantril says that one of the functions of scapegoat behavior is that "it exonerates the people themselves from any blame for their conditions" (8, p. 245).

Maslow's concept of 'dominance feeling' is similar to that of ego level though it seems to refer to a more permanent characteristic of personality (*Cf.* for instance, 35). Dominance feeling is an 'evaluation of the self' and high dominance feeling involves 'good self-confidence, self-assurance, high evaluation of the self, feelings of general capability or superiority,' etc. If a person has high dominance feeling and his own person within his life space has the properties of high value or strength, the perceptual data will be organized in such a way that they conform to that picture. If environmental changes threaten the maintenance of this cognitive representation of the own person, needs will arise to make these changes harmless by action.

With some persons there exists a tendency to keep the ego level low. They structure the attribution accordingly, as is shown by an example Maslow gives:

> For instance low dominance women can not 'take' compliments in spite of the fact of their need and hunger for them. This is because they are apt at once to discount the compliment as untrue and seek suspiciously for other motives. Often, for example, they may think the compliment is making fun of them, holding them up to ridicule, or else trying to get something out of them (35, pp. 21 f.).

Rosenzweig maintains that there exist characteristic reactions to frustration which are distinguished by the kind of attribution (43). He distinguishes between three typical reactions: in the 'extrapunitive' one the outer world is blamed for the frustration; in the 'intropunitive' one the own person is blamed, and in the 'impunitive' one the frustration is glossed over. Rosenzweig says that the kind of reaction is typical for a personality.

He relates these reactions to frustration to psychoanalytical concepts. Extrapunitive and intropunitive reactions are manifestations of outwardly or inwardly directed aggressive needs; erotic needs lead to impunitive reactions.[16]

Though these reactions are expressed in terms of needs, they can also be stated in terms of attitudes, and they are obviously related to tendencies to see the relation between the own person to the environment in a certain way.

[15] *Cf.* the examples given by Taylor (55).

[16] In later publications Rosenzweig relates the impunitive reaction to personality dissociation.

That feelings of success or failure affect attribution is suggested by an interesting theory of Janet. He says that the feelings of joy or sorrow can influence the interpretation of an experience. It is clear that he means by 'joy' (*joie*) and 'sorrow' (*tristesse*) feelings of success or failure since he defines them in the following way: They "determine the end of actions which have become useless, be it because of their success, be it because of their final defeat (*échec définitif*): these are the joys and the sorrows" (25, p. 231). That he thinks these feelings can influence attribution is shown by an example he gives: If somebody perceives a change in his possessions, the interpretation of this change will vary according to the predominant emotional state. A diminution of possession with accompanying joy will produce in the person the belief of having given something to another person; when it is accompanied by a feeling of sorrow the belief of having been robbed will arise. An increase in the possession plus joy will lead to a feeling of having robbed somebody, the same change with sorrow will make for a feeling of having received a gift (25, p. 406). He refers, of course, mainly to abnormal states.

Besides these tendencies in regard to the value level of the own person or other persons there exist doubtlessly other tendencies to structure the environment in certain ways, and we can assume with assurance that attribution is affected by all of them. As an example we mention only the tendency to see the environment as full of hostile or of benevolent intentions directed toward oneself.[17]

[17] The cognitive content 'other persons are hostile' seems to some degree, but not at all perfectly, correlated to the content 'I am of low value.' *Cf.* Maslow on the relation between feelings of insecurity and dominance feelings (37).

One could further call attention to habitual or dominant origin attributions which Nietzsche mentions:

> Not only do we try to find a certain kind of explanation as the cause, but those kinds of explanations are selected and preferred which dissipate most rapidly the sensation of strangeness, novelty and unfamiliarity—in fact the most ordinary explanations. And the result is that a certain manner of postulating causes tends to predominate ever more and more, becomes concentrated into a system, and finally reigns supreme, to the complete exclusion of all other causes and explanations. The banker thinks immediately of business, the Christian of 'sin,' and the girl of her love affair (41, p. 39 f.).

In all these cases we see that the organization is formed in such a way as to establish the best equilibrium possible under the circumstances. The properties of the parts, of origin and effect, are partly determined by factors external to the organization, stimuli and attitudes, partly by the internal requirement that origin and effect have to fit together. If the external factors favor the origin-effect unit then attribution will occur. If some external factors favor the unit, and others produce forces against unification, then the stronger factors will prevail and the weaker ones will be changed in such a way that they agree.

C. Needs and attribution

Next we have to consider dynamic factors which cannot so readily be expressed in cognitive terms. Above, we have referred to the fact that attribution can be brought about by a need for aggression. This fact is stressed, for instance, in the Yale theory of the relation between frustration and aggression. Levy (30, p. 356) presents it briefly as follows:

> In *Frustration and Aggression* the situations in which frustration occurs always

call for aggression, in terms of an attack on a frustrating agent. If no frustrating agent is present, then some object must be created for the purpose of relieving aggressive tendencies that arise in the frustrating situations. . . . This is all a familiar type of psychodynamics, namely the release of tensions arising from frustration and the use of an available object for its expression.

Here the need brings about a restructuring of the environment. A scapegoat is created by false attribution. The cognitive reorganization is explained by reference to forces toward the release of tension: the false attribution is a means for this release. At the end of section II we have tried to make it plausible why a personal origin is required as object. This reorganization could be considered as similar to 'wish fulfilment,' though here the restructuring only provides a means for the discharge of tension and does not discharge it directly. The cognitive effect of the need is brought into accord with the discharge of it in actions, as it is done, for instance, by Lewin when he says: "The need leads to a change of the environment either by a cognitive restructuring or by a change of structure through locomotion" (32, p. 109).

However, this may not be the only explanation. If we analyze other cases we find that needs can influence the cognitive environment in other directions than that of wish fulfilment.

Maslow makes it clear that if a person is insecure, the cognitive structure can be affected in two different ways. In some cases the person will "hide from himself as long as possible the conscious realization of rejection, or loss of love and respect" (37, p. 339). This is a simple case of wish fulfilment: The cognitive structure is changed in such a way that the goal of being loved appears to be attained. On the other hand, the person may see his environment in such a way that his opinion of the hostility of other persons is justified: "He will, so to speak, believe the snub and not believe the compliment" (37, p. 339).[18] This kind of distortion cannot be called wish fulfilment; rather, it is a case of what Murray called 'complementary projection.'[19] It is related to rationalization in the sense in which Bartlett uses the word, "a tendency which gives to what is presented a setting and an explanation" (3, p. 84), though it is not rationalization in the narrower sense which "consists in giving an acceptable 'reason' for an action, when the action really springs from an unacceptable cause" (55, p. 626). The perceptual data are organized so that they seem to contain a reasonable cause for the existence of the need, and not for the disappearance or discharge of the need.

It is a case of wish fulfilment when a person who is afraid of the dark pretends to be secure; it is a case of complementary projection when he sees dangerous animals in every tree stump.

Complementary projection can also occur in cases in which needs are absent. When a person has a high opinion of himself and structures his perceptions accordingly he may never leave the equilibrium of complacency for a moment, and therefore no need for regaining the equilibrium may arise. Only when objects in the environment can be responsible for unstable states (danger-insecurity, aggressive person-

[18] Maslow says that this is the reaction of the person who has given up. It is doubtful whether that is the correct interpretation since he himself says: "In every insecure person with whom the writer has worked he has always found *a continual, never dying longing for security.*" Also *"continual action toward regaining this individually defined security* was found in all cases" (37, p. 336). This seems to exclude 'giving up.'

[19] Murray, 40, p. 313. As an example he cites "the case of a guilt-ridden young man who believed that his elders were secretly condemning him."

anger) does there exist the conflict between wish fulfilment and complementary projection, between forces directed toward a stable state with discharge of tension and, paradoxically, forces directed toward the maintenance of the unstable state.

One is tempted to assume that the cognitive restructuring which occurs in complementary projection can be expressed in terms of cognitive forces more easily than in those of motivational forces.[20] The relation between dynamic states of the person and perceptual 'reasons' justifying them may be analogous to the relation between a unit and similarity between its parts. Just as a good unit requires or is produced by the fact that its members are similar, so a good fear requires or is produced by danger in the environment, and similarly a good anger is connected with a personal injury to one's own person. Though these states are disequilibria from the point of view of needs for locomotion, they are also unitary states which tend to be more complete in their own right.[21]

Whether the existence of a tension is rationalized by complementary projection, or is discharged by wish fulfilment, the effected change in the cognitive structure will often concern attribution, first, because through attribution data become relevant to the vital equilibria which refer to the relationship between one's own person and other persons, and secondly, because attribution, like the organization of an ambiguous figure, is often not univocally determined by the stimulus configuration.

[20] Sears calls Murray's conception of complementary projection 'in a sense, an example of closure' (48, p. 398). He rejects the explanation of the results of his own experiments (on ideas of reference) in terms of complementary projection. However, one can assume that his scale for the measurement of ideas of reference measures the feeling of security, and his scale for the measurement of feelings of self-criticism measures feelings of dominance. Then the results can be explained in terms of complementary projection, and they are in agreement with Maslow's findings in regard to the relationship between security and dominance feelings.

[21] Maslow speaks in a similar connection of a 'fundamental tendency in human nature of psychological inertia' (37, p. 342).

V. SUMMARY

A change in the environment gains its meaning from the source to which it is attributed. This causal integration is of major importance in the organization of the social field. It is responsible for the formation of units which consist of persons and acts and which follow the laws of perceptual unit formation. Similarity and proximity favor the attribution of acts to persons; and established person-act units make for assimilation or contrast between the parts. Tensions within the person can influence this social causal integration.

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BEHAVIOR DISORDER AS A FUNCTION OF THE RELATIVE STRENGTH OF ANTAGONISTIC RESPONSE-TENDENCIES

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It is probably true that all situations in which abnormal behavior has been observed in animals may be interpreted as *conflictful*—if not in the Pavlovian sense of a 'clash' of excitatory and inhibitory cortical processes, then at least in the sense that antagonistic adjustments are required of the animal simultaneously or in rapid succession.[1] This postulate has been employed by a great majority of workers in the field, although in some instances subjective, usually psychoanalytic, interpretations have been deductively imposed upon the data. For the most part, however, analysis of the traumatic procedures employed has not proceeded far beyond the recognition of the existence of conflict and its significance in the precipitation of experimental neurosis.

In a recent study of motor conflict in human subjects, Sears and Hovland (24) have reported evidence for an hypothesis which appears to be significant for the understanding of conflict situations. As they have stated it:

> . . . the probability of blockage as a reaction to conflict increases with the approach of the strengths of the conflicting responses to equality.[2]

A number of interesting animal investigations which contribute further evidence in support of this hypothesis have been reviewed by Miller (17) in Hunt's *Personality and the behavior disorders*. In none of these studies were disruptions of behavior more severe than blocking (failure to respond for a given period) or vacillation manifested, and it seems probable, therefore, that the *absolute* as well as the *relative* strengths of the antagonistic response-tendencies must be considered. Examination of the literature demonstrates the significance of the equivalence hypothesis for situations in which more marked behavior disorders have been reported.

At the human level, the equivalence hypothesis has found expression in early psychiatric writings. The familiar example concerns the soldier in the war situation. If fears for personal safety dominate, the man will desert; if social values and fear of punitive measures consequent to desertion dominate, he will remain and fight; but in the event that neither set of motives is strong enough to prevail, if they each make equally intense demands upon the individual, neurosis will supervene. Sears and Hovland (24) have pointed out that such reasoning is circular unless independent measures of the strength of the opposing response-tendencies are possible. Fortunately in many of the animal experiments such independent indices can be found.

Masserman's (16) recent experiments with cats provide an excellent illustration. The animals were confined in a cage and taught to take food from a covered box. When this response was learned, a sharp blast of air was directed across the box at the moment of food-taking. The conditions elicited dis-

[1] Razenkov, in Pavlov's laboratory, produced behavior disorders in dogs by presenting positive and negative conditioned stimuli in rapid succession (22, p. 343).

[2] For convenience this statement will be referred to subsequently as the *equivalence hypothesis*.

turbed behavior in many of the cats, and a consideration of the techniques employed by Masserman to accentuate or alleviate the symptoms shows that variation of the strength of the conflicting forces was the effective factor. Animals in which avoidance tendencies dominated could be made to show marked symptoms when the period of food-deprivation was increased. Leaving the door of the food box open, introducing especially desired food, or forcing the animal nearer to the food had the same effect. On the other hand, almost identical procedures tended to *alleviate* the symptoms of animals whose behavior had already been disrupted. The equivalence hypothesis fits these results nicely.

Many early experiments were performed in Pavlov's laboratory on the 'interaction of reflexes.' In 1916 Pavlov reported the results of work with Petrova on the opposition of the 'food reflex' and the 'guarding reflex' and concluded:

> You need only to increase the number of stimuli for one reflex, *i.e.*, to add weights in one pan of the scale and it sinks—one reflex suppresses the other. Depending upon the reflex to which you add the stimulus, the scale pan in which you place the additional weight, the one or the other predominates (22, p. 259).

Pavlov, as translated, referred to experimental neurosis as the consequence of disturbance in the 'balance' of excitatory and inhibitory processes (23), but it may be inferred from his experiments that he meant the failure of one or the other of these processes to achieve the dominance requisite for an ordered response. The difficult-discrimination technique which he employed to produce disruption provides a case in point. The more closely an ambiguous stimulus resembles one of the original stimuli which have been differentiated, the more likely it is to elicit behavior appropriate to that stimulus. At some intermediate point, however, it will be equally appropriate to the responses called out by both of the original stimuli, and disruption of behavior will occur. These relationships have been worked out quantitatively for a brightness-discrimination situation by Brown (2). In Pavlov's theory, consequent to the conflict there will be an 'undisputed predominance' of one set of the antagonistic processes (excitatory or inhibitiory) which will give form to the neurosis.

Despite the fact that most of the significant work in the field of experimental neurosis has been done with the classical conditioning situation, results obtained from the study of convulsive seizures in rats constitute important evidence for the equivalence hypothesis. Certain investigators (18, 19, 20, 21, 25) have objected to Maier's (10) original interpretation of the convulsive pattern as a neurotic manifestation stemming from conflict situations to which the animal is subjected, and prefer instead to regard the seizure as a reflex response to auditory stimulation.[3] Finger (3) recently has reviewed the work on the problem from this point of view. But an abundance of evidence seems to indicate that the objection is unfounded (1, 4, 6, 11, 12),[4] and the fact that some of the same evidence supports an hypothesis related to the general problem of experimental neurosis emphasizes the relation of the pattern in question to behavior disorders appearing in other situations.

Convulsive seizures can be produced in rats enclosed in a cage and exposed

[3] Hence the terms 'audiogenic' and 'audio-epileptic' which are used to designate the seizures.

[4] Perhaps the strongest evidence has been contributed by Griffiths (4) who found that seizures could be elicited when shock from the platform of the jumping stand was employed in place of auditory stimulation.

to auditory stimulation of a wide range of frequencies.[5] It can be demonstrated readily that this stimulation tends to elicit strong avoidance responses.[6] But the walls of the cage constitute a more or less effective barrier to such behavior, and a conflict exists for the animal. In this situation the rat usually shows excited exploratory behavior which may be followed either by a strong crouching pattern (representing a dominance of the inhibitory factors) or by a characteristic convulsive seizure.

There is a good deal of evidence to show that the occurrence of seizures is a function of the relative strength of the conflicting excitatory and inhibitory factors in the situation. As the size of the cage is increased, seizure frequency is reduced (11). If an activity wheel, which is less restrictive in that it permits greater scope of activity, is substituted for the cage the same results are obtained (6). On the other hand, if the possibilities for action are greatly *reduced*—by binding or harnessing the animal—a reduction in the frequency of seizures (in these instances to zero) again is obtained (4, 9, 15). With the inhibitory factor constant and the stimulus intensity increased, formerly normal animals begin to show seizures (21). It would seem to follow from this evidence that disruption stems from an *equation* of the opposing response-determinants.

The importance of restriction in the production of experimental neurosis has long been emphasized (7, 8, 10, 12, 13, 26). It has been found that an animal will tend to avoid responding in terms of stimuli which are difficult to discriminate unless limitations are imposed upon the scope of its behavior. The following quotation from Witkin provides a representative statement of this point of view:

> . . . the animal's typical mode of response to a conflict which has become very difficult is to resort to extraneous types of behavior. The severity of the disturbance produced by a conflict in different types of situations is related to the degree of restriction imposed by the given situation upon the animal's field of action. A more restricted field of action curtails the opportunity for extraneous responses, focalizes the conflict prepared by the experimenter, and thereby makes an adequate adjustment more difficult (26, p. 73).

In the light of the equivalence hypothesis restriction must be given a less central role.

In the discrimination situation conflict may exist at two levels—at the *stimulus level* and at what may be called the *situational level*. Stimulus conflict induces avoidance behavior which can be curtailed by various physical restraints, and a 'forcing agent' such as shock or compressed air usually is introduced to compel reaction to the stimuli. Conflict now exists at the situational level. In the classical conditioning situation an additional compelling stimulus is not required because the stimuli to be discriminated apparently call out the learned responses more directly (13). That the probability of disruption does not vary concomitantly with degree of restriction may be seen in the fact that *extreme* restriction may operate to *prevent* disruption, not only in the seizure situations described above, but in the Pavlov frame as well. While Parmenter (5, p. 289) reported the disappearance of symptoms when his neurotic sheep was allowed the freedom of a pen (5 feet square) instead of being confined in the frame, Marcuse and Moore (14), also working at the Cornell Laboratories, found that tantrum behavior could be induced in a formerly

[5] It was believed originally that only higher frequencies were effective, but seizures may be produced as well by tones of low frequency (18).

[6] In the experimenter as well as in the animal.

quiet pig by *loosening* the animal's harness. Restriction, therefore, is important only as one of the antagonistic influences operating upon the animal and contributes to disruption only when its magnitude is equivalent to that of the opposing influences.

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